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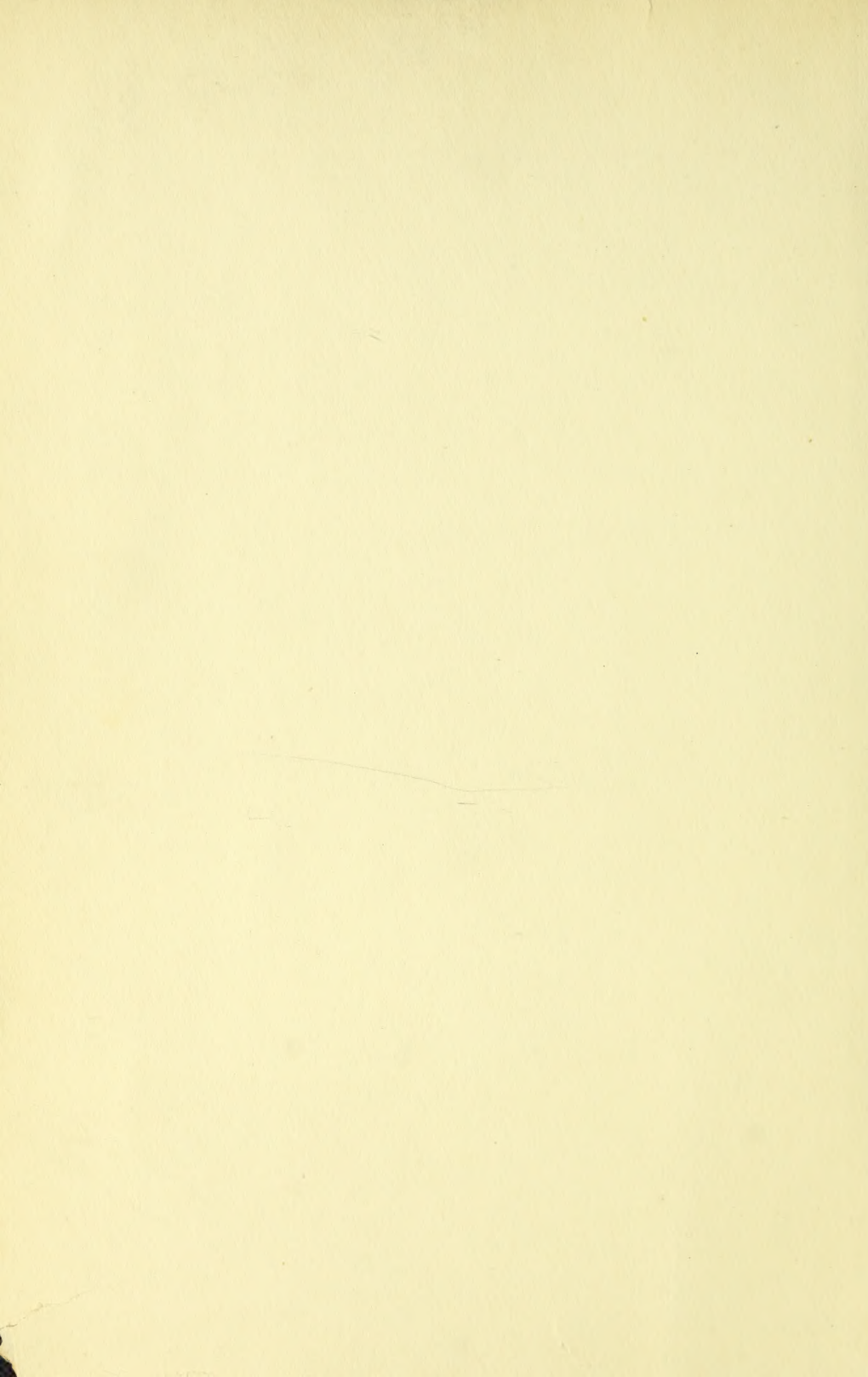
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EDITED BY

JAMES ROWLAND ANGELL, UNIVERSITY OF CHICAGO

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Index*)

JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY (*Review*) and

ARTHUR H. PIERCE, SMITH COLLEGE (*Bulletin*)

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# THE Psychological Monographs

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## The Factors that Influence the Sensi- tivity of the Retina to Color: A Quantitative Study and Methods of Standardizing

By

GERTRUDE RAND, PH.D.

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## PREFACE.

The following study, practically as is here presented, was submitted to the Faculty of Bryn Mawr College in May 1911 in partial fulfillment of the requirements for the degree of Doctor of Philosophy. It is the outgrowth of a series of studies dealing with the phenomena of color vision that was begun in 1908 by the writer working under the direction of and in collaboration with Professor C. E. Ferree of Bryn Mawr College. In order to show in what way the present study, which deals with the formulation of a technique for investigating color sensitivity, is the logical outcome of the initial studies in the series, and is, moreover, required for the completion of the later studies, a brief résumé will be given of the work undertaken in the investigations preceding and following it.

The first of these studies entitled: *Colored After-image and Contrast Sensations from Stimuli in Which No Color Is Sensed*, was published in the *Psychological Review*, 1912, XIX, pp. 195-239. As is shown by the title, the article deals with the conditions under which colored after-image and contrast sensations may be aroused from stimuli in which no color is sensed. A formulation of these conditions, together with allied fusion and limen experiments, shows the phenomenon to be a peculiarity of the inhibitive action of brightness upon color. Brightness fused with color inhibits its saturation. With the exception of the region just within the limits of sensitivity for two colors, the following may be stated roughly as a law of this action for all colors and all parts of the retina: white inhibits most, grays in the order from light to dark next, and black least. This law was generalized from the results of fusion and limen experiments in a large number of meridians of the retina. In accord with this law, color may be obtained in the after-image when none is sensed in the stimulus when an unfavorable brightness quality is fused with the stimulus color and a favorable one with the after-image color. The technique for securing these conditions

for the after-image sensations in central and peripheral vision, for the contrast sensations in central vision, and for the phenomenon which we have called the Purkinje-Brücke phenomenon, is described in detail in the paper. But the study is not quantitative. The results do not show, for example, that the inhibition of the stimulus color has no effect upon the after-image. They show merely that, working near the limen, the stimulus color may be inhibited and the complementary color still be sensed in the after-image.

In order to determine as accurately as possible to what degree, if at all, the intensity of the after-image excitation is decreased by adding to the stimulus color a brightness excitation unfavorable to its saturation, the second study of the series was begun in 1909. It was entitled: *The Fusion of Colored with Colorless Light Sensation: The Physiological Level at Which the Action Takes Place*. An abstract of this article was published in the *Journal of Philosophy Psychology and Scientific Methods*, 1911, VIII, pp. 294-297. The full report will shortly be published in the *Psychological Review*. The results of this study have a twofold bearing. (1) They make plain once for all why it is possible to obtain color in the after-image when none is sensed in the stimulus, for they show that the intensity of the after-image excitation is not decreased at all by adding to the stimulus color a brightness excitation unfavorable to its saturation. (2) They throw some light on the broader problem presented by the fusion of brightness and color. By serving to indicate the level at which this action takes place, they help, for example, to explain a number of somewhat puzzling phenomena attendant upon the fusion of brightness with color, in case of positive, after-image, and contrast sensations. This action takes place apparently posterior to the seat of the after-image and contrast processes and the cancelling action of the complementary colors. There are two effects of the fusion of brightness with color, both of which are pressed into service in drawing the above conclusion: (1) it reduces the saturation of the color sensation; and (2) it changes the quality or tone of certain colors. This conclusion is based on the following lines of argument:

(1) When the color of the stimulus is inhibited by the addition of a brightness excitation, the intensity of the after-excitation, judged in terms of the duration of the after-image, is not affected by this excitation. (2) When the tone of the color aroused by a given stimulus is changed by the addition of a brightness excitation, the color of the after-image does not undergo a complementary change. (3) When the saturation of the inducing color is inhibited by the addition of a brightness excitation, the saturation of the contrast color is not affected by the change. (4) When the tone or quality of the inducing color is modified by adding a brightness excitation, the tone of the contrast color is not determined in the complementary direction. (5) When a given color is inhibited by the addition of a brightness excitation, its power to cancel the complementary color is not altered. (6) When the tone or quality of a color is altered by the addition of a brightness excitation, the tone or quality of the color required to cancel it is not affected by the change. (7) When the tone or quality of a color has been altered by the addition of a brightness excitation, the color component added can not be cancelled by mixing with the original color a color complementary to this component. Since, then, this fusion affects the positive and not the negative excitation and does not affect the cancelling action of the complementary colors, the conclusion is drawn that it takes place at some physiological level posterior to the seat of the after-image and contrast processes and to the cancelling action of the complementary colors. This study deals, however, only with the measure of the effect of the fusion of brightness and color as it occurs in central vision.

In order to extend the investigation to the peripheral retina, a third study was begun in June 1911. Its object was as follows. (1) It was planned to determine the effect of the fusion of brightness with color at a number of points from the center to the periphery of the retina, and to see how far the following points can be explained in terms of this action: (*a*) the influence upon the limits of color sensitivity of the brightness of the surrounding field and of the preëxposure; and (*b*) the color changes that occur in passing from central to peripheral vision. (2) A

comparative study was to have been made of the chromatic and achromatic phenomena of the peripheral retina. Both of these sets of phenomena for the peripheral retina show a number of striking differences from the phenomena of the central retina. Some of these points of difference are as follows: (*a*) There is considerable difference in the action of the achromatic qualities on color. (1) They inhibit or reduce the saturation of color much more strongly in the peripheral than in the central retina. (2) Of the achromatic qualities, white inhibits all colors the most strongly and black the least strongly in the central retina, while near the limits of sensitivity in the peripheral retina, black inhibits red and yellow the most strongly and white the least strongly. (3) The change in the tone or quality produced by adding white or black is much more pronounced in the peripheral retina and is often in a different direction. For example, black added to yellow in daylight illumination in the central retina turns it towards green; while in the peripheral retina the change is towards red. (*b*) Varying the brightness of the surrounding field has more effect on colors in the peripheral than in the central retina. (*c*) Exhaustion to color takes place more rapidly in the peripheral than in the central retina, that is, the change of saturation per unit of time is faster. (*d*) The colored after-image is of very short duration in the peripheral retina, but in proportion to its duration, it is much more saturated than the after-image of the central retina. (*e*) Some of the differences with regard to the achromatic phenomena are as follows. There is a very strongly increased sensitivity to contrast and to flicker; adaptation or exhaustion occurs very rapidly; the after-image is quickly aroused, is relatively very intensive, and in proportion to its intensity lasts a very short time; and so on. These points of difference raise the question how far we need go in assuming a different mechanism for the two parts of the retina. The purpose of our investigation was to have been primarily to determine how many of these differences are due at least in part to the difference in the state of brightness adaptation of the central and of the peripheral retina. (3) Maps were to have been made showing the sensitivity of the eye to the different

colors for three kinds of background (white, black, and gray of the brightness of the stimulus color). These three backgrounds were selected because they represent the extreme situations with regard to achromatic induction: maximal black induction, maximal white induction, and no induction; and, therefore, represent the best conditions that can be obtained for the study of the effect of the brightness of the surrounding field upon the local sensitivity of the retina. A sufficient number of meridians were to have been worked over to give an accurate outline of the zones of sensitivity for the three kinds of background used. Gradients were to have been established showing the falling off in sensitivity from the fovea outwards. Also the changes in color tone were to have been determined from point to point for all the backgrounds. Both sets of determinations were to have been made by matching in central vision what is seen in peripheral vision. The object of this investigation was to have been to give a complete representation of the sensitivity of the entire retina, quantitative and qualitative, in terms that are more or less familiar to all, namely, the sensation values of the central retina. It was found, however, that the large M. V. occurring in the work from observation to observation rendered the extended comparative investigation planned impossible. The original plan had, therefore, for the time to be abandoned, and the present study was undertaken.

This study aims (1) to determine what are the factors that influence the sensitivity of the retina to color; (2) to make a quantitative examination of the factors extraneous to the stimulus; and (3) to provide methods for their standardization. For the sake of historical continuity, the study is preceded by an historical and critical résumé of the analyses of factors influencing color sensitivity that have been made up to this time and of the attempts to standardize. It may be stated in passing that with the control of factors rendered possible by this study, the original plan of work has been resumed and in part completed. It will be published in the near future.

There remains to be mentioned the relation of the present study to the final one of the series. The latter has developed

from the historical and critical résumé mentioned above of the investigations that have been made to determine the factors that influence the sensitivity of the retina to color. In the course of this discussion various deficiencies have been pointed out in the methods used by previous investigators in their attempts to control these factors, and ways have been devised to correct these deficiencies. The factors that influence the sensitivity of the retina to color may be divided into two classes: those pertaining to the stimulus, and those extraneous to the stimulus. The experimental part of the present study is especially directed toward making a quantitative estimate of the latter set of factors under various typical conditions obtaining in the investigation of color sensitivity and toward securing effective methods of control. No concern is had, however, to standardize the factors pertaining to the stimulus any farther than is necessary to accomplish this purpose. The more effective standardization of these factors will form the subject of our future work. The question of intensity will be taken up first. It has been shown in the historical part of the present study that this factor has been most inadequately handled by previous investigators. In determining the comparative sensitivity of the retina to the different colors, for example, either no account has been taken of the different intensities of the colors used, or incorrect methods have been employed of equalizing these intensities. In no case has the determination been made in terms of units that can be compared. It is the writer's purpose to make an exhaustive determination of the sensitivity of the retina to the different colors in terms of such units. The comparative limits of sensitivity will be determined in a number of meridians with stimuli equalized in energy, and the limens and the j.n.d's. of sensitivity at different degrees of intensity will be determined in terms of radiometric units at various points from the center to the periphery of the retina in the different meridians. This investigation in fact is now in progress. A preliminary statement of the plan of this work has already been published by the writer in collaboration with Professor Ferree (*American Journal of Psychology*, 1912, XXIII, pp. 328-332).

From the above discussion of the place of the present study in the series, it will be evident to the reader how extensively this study has been due to the instruction, guidance, advice, and assistance of Professor Ferree, and how great a debt of gratitude the writer owes him. In stating his share and collaboration in the studies preceding and following this in the series, I can indicate perhaps more fully than in any other way the share he has had both directly and indirectly in the production of the present study.



## I. INTRODUCTION.

In no branch of psychological optics does one find such varied and contradictory results as in the work on the color sensitivity of the peripheral retina. This is doubtless due in minor part to the intrinsic difficulty of the indirect vision observation, but in major part it is due to the lack of adequate standardization of the factors that influence the local sensitivity of the peripheral retina. These factors may be divided into two classes: (*a*) those pertaining to the stimulus, or the source of light; and (*b*) those extraneous to the source of light. In the former class may be included the size, intensity, and brightness of the stimulus; in the latter, the preëxposure or what the eye has rested on before being exposed to the stimulus, the surrounding field, and the general illumination of the visual field.<sup>1</sup> The work of standardization thus far has been directed largely towards the factors in the former class. Of the factors in the latter class, attempts have been made, as will be shown later in the discussion, to standardize only the influence of surrounding field. The recognition of the importance of this factor came relatively late in the development of the technique of the subject. It was at one time thought that the use of the perimeter and the dark-room provided ideal conditions for testing the local sensitivity of the peripheral retina, because by this means the local area alone was stimulated by light, hence it was thought that the influence of the surrounding field was eliminated. We know now that these conditions were not so ideal as they seemed, that a dark- as well as a light-

<sup>1</sup> In case the colored light is obtained by reflection from a pigment surface, some exception may be taken to the above classification, for unless some especial device be used to illuminate the pigment surface, the intensity of the stimulus will depend upon the degree of the general illumination of the visual field, and the brightness of the stimulus will also, to a certain extent, be dependent upon the general illumination. In such a case, these factors would have to be included in both classes. If on the other hand the colored light is obtained by means of standard filters, or from the spectrum, the illumination of the visual field will exercise its influence entirely independently of any effect on the stimulus.

adapted retina influences by contrast the sensitivity of the area stimulated. The use of the perimeter and dark-room accomplishes, then, but a very small part of the purpose for which it was intended. Instead of eliminating altogether the influence of the brightness of the surrounding field, it makes only one phase of it constant. It standardizes by giving us one state of brightness-adaptation alone, namely, the adaptation of the dark-room.<sup>2</sup> The campimeter was devised especially to correct this deficiency. Its purpose is to control and standardize the influence of the brightness of the surrounding field when one is working with a light-adapted retina. But the campimeter, like the perimeter, has accomplished only in part the purpose for which it was intended. It standardizes the influence of the surrounding field for one degree of illumination only, because the influence of the campimeter screen changes markedly with changes in the illumination of the visual field. There are two reasons for this. (*a*) A brightness match between the colored stimulus and the gray of the surrounding field made at one illumination will not hold at another. And (*b*) the sensitivity of the retina to brightness induction changes markedly with changes in the general illumination. This latter point is especially true in the peripheral retina where changes which are too small to be detectable by any current photometric device produce quite a noticeable change in the amount of induction between two surfaces of different brightness. The campimeter, then, is almost useless as an instrument of precision, unless the general illumination can be rendered constant or some means can be devised for standardizing the observation for changes of illumination. No satisfactory method has as yet been obtained for keeping the illumination of a room by daylight constant. To keep it constant presupposes what has not as yet been provided, namely, a sensitive means of measurement. Constancy may be approximated by artificial illumination,

<sup>2</sup> As will be shown later in the paper, neither the influence of surrounding field nor of preexposure can be eliminated when the observation is made in the dark-room. The influence of these two factors can be eliminated only by working in a light-room of constant intensity of illumination, and by using a preexposure and a surrounding field of the brightness of the color used for the stimulus.

but no artificial source has yet been devised which gives a light that approaches average daylight<sup>3</sup> sufficiently closely in composition to warrant its use in color work. Of the various sources of light the Moore Tube comes nearest to doing this, but spectrophotometric and colorimetric determinations show that the light from it contains an excess of blue<sup>4</sup> and, therefore, although it has been adopted by various textile concerns for use in color matching, its substitution for daylight can scarcely be recommended for the more exact requirements of color optics. Ives and Luckiesh<sup>5</sup> attack the problem of producing artificial daylight from another side. By their subtraction method they claim to have gotten the closest approximation to average daylight yet attained. They aim to cut out by absorbing screens the excess of red and yellow in artificial light due to the comparatively low temperature of artificial illuminants. Tungsten lamps are used by them as the source of light, and two kinds of commercial glass approximating in their absorptive action cobalt blue and signal green are used as screens. In order to correct for the pronounced band of yellow-green transmitted by the cobalt blue, a film of gelatine dyed with rozeine is also used. Although according to comparative measurements made by Ives and Luckiesh the light thus gotten is the closest approximation to average daylight yet obtained, still it shows a deficiency of 15% in the green and about 25% in the blue. Moreover, the spectrum of this light does not show the brightness distribution of the spectrum of daylight. Since the absorbing screens cut down the light emitted by the tungsten lamp to 15% of its original intensity, the spectrum of

<sup>3</sup> For results of measurements of the color values of average daylight, see Nichols, E. L. *Transactions of the Illuminating Engineering Society*, 1908, III, p. 301. Ives, H. E. The Daylight Efficiency of Artificial Illuminants. *Transactions of the Illuminating Engineering Society*, 1909, IV, pp. 434-442; Color Measurements of Illuminants. *Transactions of the Illuminating Engineering Society*, 1910, V, pp. 189-207.

<sup>4</sup> See Ives, H. E. Color Measurements of Illuminants. *Transactions of the Illuminating Engineering Society*, 1910, V, p. 206; and Rosa, E. B., quoted by Moore, D. McF. A Standard for Color Values. *Transactions of the Illuminating Engineering Society*, 1910, IV, p. 224.

<sup>5</sup> Ives, H. E. and Luckiesh, M. Subtractive Production of Artificial Daylight. *Electrical World*, 1911, LVII., pp. 1092-1094.

the light finally given out shows the brightness distribution characteristic of lights of low intensity, unless the original light-source is of extremely high candle-power.

We seem thus compelled either to give up the investigation of the sensitivity of the retina for daylight illumination, or to devise some means of keeping this illumination constant. At an early stage in the study of the color phenomena of the peripheral retina begun four years ago and still in progress in the Bryn Mawr Laboratory, the writer was compelled to take into account the influence of the changes in the illumination of the visual field upon the color observation. The changes of illumination that took place from day to day, the progressive changes during the day, and the many sudden changes even in the course of an hour, rendered any constancy, or close reproduction of results entirely out of the question. The consideration of this factor led in turn to a general study of the conditions that influence the color observation. It is the purpose of this paper to report the results of that study. The report will take the following form.

(1) A résumé and criticism will be given of previous studies of factors, and of attempts to standardize. (2) The color observation will be reexamined for the factors that influence its results, and a study of these factors will be made with the following points in view: (*a*) Their influence will be measured under various typical conditions obtaining in the work on color sensitivity. (*b*) Their effect on the limen of color at different points in the retina and on the limits of color sensitivity will be determined. (*c*) An explanation based on the conclusions drawn from (*a*) and (*b*) will be made of the results of other experimenters and of the contradictions found in these results. (3) From this study of the influence of the factors, it will be determined what factors need to be standardized in the various kinds of work on color sensitivity and methods will be devised for their standardization.

In the latter part of the work especial attention will be given to the effect of general illumination and of local preëxposure. The writer finds these to be the two most important factors extraneous to the source of light that influence the results of the color obser-

vation, and yet, so far as she is able to determine, up to this time no attempt worthy of more than passing consideration has been made to standardize either factor in investigations of color sensitivity. In fact, it can scarcely be said that either has been included in the list of factors by any previous writer. The effect of the general illumination has received only casual mention by Ole Bull and a few others, and the brightness of the preëxposure has not been clearly recognized as exerting any influence whatever.

## II. HISTORICAL AND CRITICAL.

### A. FACTORS THAT HAVE BEEN FOUND TO INFLUENCE THE SENSITIVITY OF THE RETINA TO COLOR.

#### I. *Size of the Stimulus.*

An increase in the size of the stimulus is generally considered to be equivalent in some proportion to an increase in intensity.<sup>1</sup> It is but natural, then, to think that an increase in the size of the stimulus would both lower the limen of sensitivity and extend the limit of the zone within which a given color can be sensed. The question with regard to the limits of sensitivity is, however, not so simple as it seems. In the first place, the limit of the zone may not be extended, because the retinal sensitivity may fall off so rapidly at the point worked upon, that the increase of stimulation is not sufficient to overweigh the loss. In the second place, the effect of the increase of area may depend to some extent upon the area of the original stimulus. For example, fatigue is set up so easily with very small stimuli that an increase up to a certain point is advantageous, while, on the other hand, the outer margin of large stimuli may extend so far into the zone of relative insensitivity that a further increase of area becomes ineffective. In the third place, the effect may vary with the meridian of the retina investigated. Two reasons may be assigned for this variation. (a) We should expect the effect to be in some measure proportional to the rapidity with which the retina falls off in sensitivity from the fovea to the periphery. For example, in the temporal and lower

<sup>1</sup> Raehlmann, E. Ueber Farbenempfindung in den peripherischen Netzhautpartien in Bezug auf normale und pathologische Brechungszustände. Inaug. Diss., Halle, 1872.

While the work of Raehlmann and others shows in general the truth of the above statement, no systematic determination of the exact relation of change of area to change of intensity has yet been made. This determination for the sensations aroused both by white and colored light is now in progress in the Bryn Mawr laboratory.

meridians, where the sensitivity falls off sharply, we should expect little if any effect; while in the nasal and upper regions, where the decrease is much more gradual, we should expect considerable effect. (*b*) In the nasal and upper meridians, the limits of sensitivity extend much farther toward the periphery than in the temporal and lower meridians. There is in these meridians, then, as the limits of sensitivity are approached, a relatively greater shrinkage in one dimension of the stimulus, owing to the greater angle of excentricity, than occurs in the temporal and lower regions. In proportion as this shrinkage causes a shortening of one dimension of the stimulus, it adds to the range of areas over which an increase is of advantage for extending the limit of sensitivity.

A survey of the literature on peripheral vision shows that the size of the stimulus was early recognized as one of the factors influencing the sensitivity of the peripheral retina. In fact, the first investigation of peripheral sensitivity was made to determine the effect of the size of the stimulus. This work done by Hueck<sup>2</sup> in 1840, may be considered as pioneer, for although Troxler<sup>3</sup> and Purkinje<sup>4</sup> had previously mentioned the phenomena of peripheral vision, they had made no systematic attempt to investigate these phenomena. Hueck's object was primarily to study the effect of increase in the size of the stimulus upon the limits of the field of vision. Using gray paper stimuli of very small area, he observed the effect on the limit of vision (*a*) when their objective size was increased, and (*b*) when their apparent size was altered by a decrease in their distance from the observer, that is, by enlargement of the visual angle. He found that an increase in size produced in either of these ways caused a widening of the field of vision for that quality of stimulus. The investigation was also extended to color. Fig-

<sup>2</sup>Hueck, A. Von den Grenzen des Sehensvermögens. Müller's Archiv, 1840, p. 95.

<sup>3</sup>Troxler, D. Ueber das Verschwinden gegebener Gegenstände innerhalb unseres Gesichtskreises. Ophthal. Bibliothek herausgegeben von Himly u. Schmidt, Jena, 1804, I, 2. pp. 1-53.

<sup>4</sup>Purkinje, J. Beiträge zur Kenntniss des Sehens. 1823, I, p. 76; 1825, II, p. 14.

ment papers were used. This investigation showed that the limits of color sensitivity also are influenced by the size of the stimulus. An interesting table was compiled which shows that by altering either the size of the stimulus or the visual angle it subtends, the limit of sensitivity can be made to vary by amounts equal to  $1^\circ$  over a wide range of the retina. Hueck's conclusion, that the limits of color vision are influenced by the size of stimulus, was confirmed by Aubert<sup>5</sup> in 1865. Five years later it was contradicted by Woinow.<sup>6</sup> Woinow worked in the dark-room, using for stimuli colored glasses illuminated by a shaft of sunlight of variable extent. He claimed that "die Grenze immer dieselbe ist, ohne Rücksicht auf die Grösse der Pigmentfläche, wenn die Gesichtswinkel nicht von der Mitte sondern von dem dem Auge zugekehrten Rande der Pigmentfläche berechnet werden." No information is given as to the size of stimuli employed. Krükow,<sup>7</sup> repeating Woinow's precaution of measuring the angular distance to the inner edge of the stimulus rather than to the middle, confirmed the conclusion that the boundaries of the color zones are absolute, within certain limits of size of stimulus. His stimuli were 3, 6, and 9 mm. square. Aubert<sup>8</sup> in 1876, repeated the observations recorded in his earlier work. Colored squares with sides varying from 1 mm. to 32 mm. placed at a distance of 20 cm. from the eye were used as stimuli. The results he obtained led him to believe that the size of the stimulus is a factor in determining the limits of sensitivity. He writes: "Die Grösse des farbigen Objectes massgebend ist für die Entfernung vom Centrum, in welcher es noch farbig empfunden wird. Die gegentheilige Behauptung Woinow's . . . muss ich nach vielfacher, wiederholter Untersuchung für falsch erklären." He mentions the precaution used by Woinow as to the measurement of the angular distance, but does not definitely state that he himself took this precaution. Raehl-

<sup>5</sup> Aubert, H. *Physiologie der Netzhaut*. Breslau, 1865, p. 121.

<sup>6</sup> Woinow, M. *Zur Farbenempfindung*. A. f. O., 1870, XVI, p. 219.

<sup>7</sup> Krükow. *Objective Farbenempfindung auf den peripherischen Theilen der Netzhaut*. A. f. O., 1874, XX., pp. 255-296.

<sup>8</sup> Aubert, H. *Physiologische Optik*. Leipzig, 1876, pp. 541-544.

mann,<sup>9</sup> Schön,<sup>10</sup> Schirmer,<sup>11</sup> and Briesewitz,<sup>12</sup> all agree with Aubert; but they also have not mentioned their method of measurement.

In Tschermak,<sup>13</sup> however, we find an investigator who has observed Woinow's precaution and yet has obtained results that are contradictory to Woinow's. He investigated the factors which condition the colorless vision of the peripheral retina, and showed that neither the red-green nor the totally color-blind zones of the normal retina are invariable in extent. Size of stimulus was found by him to be one of the factors that determine the breadth of these zones. This he demonstrated on the Hering apparatus for investigating the color sensitivity of the peripheral retina, an apparatus consisting of a lampimeter screen with an opening behind which the stimulus is placed. Tschermak's screen was of gray paper. The size of the stimulus-opening was regulated by means of two gray slides which widened the opening either on the side within the visual field and toward the fovea, or on the other side toward the periphery. Using a small stimulus-opening, he determined the degree of excentricity at which *Urgrün* and *Urroth* appeared colorless. He then widened the stimulus-opening toward the fovea and found that the color was sensed. No conclusion, however, can be drawn from this because he had extended the inner margin of the stimulus into the region sensitive to color, hence the sensation aroused may have been due to that cause rather than to the increase made in the area of the stimulus. He next widened the original stimulus-opening toward the periphery. This caused an increase in the area of the retina stimulated, without extending the margin of the stimulus into the field sensitive to color. Since in this case also the color was sensed, Tschermak concludes that the

<sup>9</sup> Raehlmann, E. loc. cit.

<sup>10</sup> Schön, W. Ueber die Grenzen der Farbenempfindung in pathologischen Fällen. Klinische Monatsblätter, 1873, p. 171.

<sup>11</sup> Schirmer, R. Ueber erworbene and angeborene Anomalien des Farbensinns. A. f. O., 1873, XIX, p. 194.

<sup>12</sup> Briesewitz. Ueber das Farbensehen bei normalem and atropischem Nervus Opticus. Inaug. Diss., Greifswald, 1873.

<sup>13</sup> Tschermak, A. Beobachtungen über die relative Farbenblindheit in indirectem Sehen. Pflüger's Archiv, 1890, LXXXII, pp. 559-560.

limits of color sensitivity are influenced by the area of the stimulus. It is in the second method of increasing the area of the stimulus that Tschermak took the precaution mentioned by Woinow relative to the measurement of the angle of excentricity.

But it is obvious that the method of measurement need not have been the only cause of variable results in work of this kind. The meridian tested may have been, as we have already suggested, a second cause. That certain regions of the retina react differently to an increase in the area of the stimulus, is noted in a brief paragraph by Kirschmann.<sup>14</sup> Using stimuli of 28, 40, and 58 mm. in diameter, he found that the color sensitivity of the peripheral retina is dependent to different degrees in different meridians upon the size of the stimulus. In the lower and temporal meridian, the zone sensitive to each color was widened very slightly by increasing the area of the stimulus, and never beyond certain limits. On the upper and nasal parts of the retina, however, the possibility of widening the zones by this means seemed to be, he says, unlimited. Now we know that Woinow and Krükow obtained their results on the temporal meridian. Tschermak, however, does not state what region he investigated. If he worked in the nasal region, his conclusions may be reconciled with those of Woinow in the light of Kirschmann's work. These variations in the effect of area in different regions of the retina are no doubt due largely to the difference in the rapidity with which sensitivity falls off from the center to the periphery of the retina along the several meridians. Where the decrease is gradual, as is the case in meridians that have wide limits of sensitivity, more effect might be expected than where the sensitivity decreases rapidly.

A third cause of the variable results recorded may have been the range of size of stimuli employed. The retina fatigues easily to very small stimuli; hence an increase in size up to a certain point is advantageous. On the other hand, the margins of very large stimuli may extend so far into the zone of insensitivity that a further increase is ineffective. Krükow no doubt

<sup>14</sup> Kirschmann. A. Die Farbenempfindung bei indirectem Sehen. Philos. Studien, 1893, VIII, p. 612, 613.

referred to this fact when he said that color sensation is independent of the size of the stimulus, but only within certain limits. An interesting conclusion reached by Abney<sup>15</sup> may also be mentioned in this connection. He wished to determine at what intensity the different colored spectral lights were brought below the limen of sensation both in central and at various points in peripheral vision. He found, however, that the intensity of the stimulus is not the only factor to be considered. A stimulus 2 inches in diameter, for example, was seen at a lesser intensity than a stimulus  $\frac{1}{2}$  inch in diameter. He further found that it is not the area, but the shortest dimension of the stimulus, vertical or horizontal, which determines the intensity required to render the stimulus subliminal.

The following table has been compiled from the results of his work in central vision.

<i>Stimulus</i>		<i>Relative Intensity</i>	<i>Value of Light</i>
disc	.95 in. diameter	234	97.4
square	.84 in. x .84 in.	216	139.2
rectangle	1.68 in. x .42	152	495.2
square	.84 in. x .42	154	478.4

The areas of the disc, the square, and the first rectangle are equal, but the rectangle which has the shortest dimension, requires 400 units more of light intensity than does the disc in order to be made just subliminal. The fourth stimulus has half the area of the third, but their shortest dimensions are equal, and accordingly the same amount of light is required to render them both just subliminal. The experiments were extended by Abney to the peripheral retina, and the conclusion was again reached that the shortest dimension of the stimulus and not its area determines the reduction in intensity necessary to render the stimulus just subliminal (p. 183). Since Abney found further that "there is a simple connection [relation] between the intensity of the stimulus color and the extent of the color field," we may infer that he would have us conclude that the extent of the color field is also influenced by the shortest dimension of the stimulus.

The present status of this point may be summarized as follows:

<sup>15</sup> Abney W. de W. *The Sensitiveness of the Retina to Light and Colour*. Philos. Trans., 1897, CXC, Ser. A, pp. 169-171.

Within a certain range of dimensions and particularly for certain regions of the retina, the size of the stimulus is an important factor in determining the extent of the color zones. And with regard to size, the shortest dimension of the stimulus and not its area is, according to Abney, the determining factor. Care must be taken, therefore, to measure accurately the size of the stimulus used in peripheral investigation, also its distance from the observer, and to keep these measurements uniform throughout the investigation.

2. *Intensity and Brightness or White-Value of the Stimulus.*

(a) *The confusion that has arisen with regard to the meaning of intensity and of brightness, and its effect upon the development of methods of working.*

Before we attempt to discuss the influence of the intensity and the brightness of the stimulus upon the limits of color sensitivity, some attention should be given to a definition of terms. The need for this will be shown by a brief examination of the literature on these subjects. A great deal of confusion as to terminology seems to exist, and not a little misinterpretation of fact seems traceable to this confusion. The greater part of the confusion arises from the use of the word *intensity*. This term has been employed at various times to indicate (a) the energy of a beam of spectral light homogeneous as to color; (b) the white-value of a color; (c) the saturation of a color; and (d) the energy of light-waves reflected from a pigment surface as conditioned by the general illumination of the visual field. This equivocal use of the term has now and then apparently led to a wrong interpretation of results, and this in turn to the modification of experimental technique. An example of this is found in the work done by Baird in the Cornell laboratory on "*The Color Sensitivity of the Peripheral Retina.*"<sup>16</sup> In his review of the literature, Baird finds data that lead him to assume that an equation of the white-values of the stimuli employed is essential for a determination of the relative extent of the retina's sensitivity to the different colors. Apparently these data are derived

<sup>16</sup> Baird, J. W. *The Color Sensitivity of the Peripheral Retina*. Carnegie Institution of Washington, 1905.

mainly from three sources: (a) from a study of the color sensitivity of the peripheral retina made by Aubert; (b) from a study by Abney of the effect of changes in the energy or intensity of spectral light upon sensation; and (c) from a study of the limits of color sensitivity made by Landolt.<sup>17</sup> An examination of the investigations made by these men shows, however, that Baird's conclusion is apparently based upon a loose construction put upon the meaning of certain terms. The most striking example of this, as we shall see, results from the interpretation given by Baird to the term *intensity*. Baird uses *intensity* to indicate *luminosity* and, as we shall show, he also uses *luminosity* interchangeably with *brightness* or *white-value*. Landolt and Abney, on the other hand, from the results of whose investigations of the effect of intensity on color sensitivity Baird largely draws his conclusions as to the need of equating the white-value of his stimuli, clearly use the term *intensity* to mean the *energy* of the light-waves coming to the eye.

According to Baird, the first mention of the need to equate in brightness was made by Aubert. Baird writes: "His [Aubert's] results may be summarized as follows:

"1. The brightness of the background has a most pronounced influence upon the extension both of the color sensitivity and of the brightness sensitivity.

"2. The extension of the color zones increases with increase of area of stimulus.

"3. The color sensitivity decreases at very different rates upon different retinal meridians.

<sup>17</sup> Baird claims to derive authority also from the work of Raehlmann, Klug, Chodin, Bull, Hess, and Hegg. We have considered that this authority is derived mainly from Aubert, Landolt, and Abney, however, because Baird discusses the results of these three men and to some extent their methods of working, giving several sentences to show that their work points out the need for equation of the white-values of the stimuli employed to investigate color limits. To the other men from whom he claims to derive authority, Baird devotes merely a sentence to each which states, in case of Raehlmann and Klug, that they had found that the color limits vary with changing brightness of stimulus; in case of Chodin, that he believed that brightness equation was necessary; in case of Bull, Hess, and Hegg, that they had equated the white-values of their stimuli. The discussion of these cases will be taken up later in the paper (see pp. 51-53).

"4. The transitions of color tone are as follows: Red passes through reddish-yellow and yellowish-gray to gray; green becomes yellowish, while yellow and blue undergo no change of tone, but decrease in saturation and finally appear gray.

"5. The relative extension of the color zones can not be determined with any degree of accuracy. Since the width of the color zone is a function of the luminosity of the stimulus, the color-stimuli employed in the determination of comparative retinal limits must all be equated in brightness.

"6. There is a close analogy between the functioning of the central and peripheral parts of the retina."<sup>18</sup>

Baird seems to derive his authority for the need to equate in brightness, so far as Aubert is concerned, from the fifth of these points of summary. From the wording of the text it is impossible to state the exact source of Baird's quotation since he bears himself out in his summary only by a general reference to a long list of Aubert's articles on vision.

But the organization of this summary is so closely akin to that given in the *Physiologische Optik*,—the only difference being in the omission by Baird of the third item in the *Optik* summary,—that one seems justified in asserting that this work contains the source of the statement quoted above. In no other of Aubert's articles are all of the points mentioned by Baird touched upon. The earlier articles are narrower in scope than the *Optik* and treat of fewer factors.

Aubert's statement of results in the *Physiologische Optik* is as follows: "Durch meine Versuche wurde festgestellt

"1. der grosse Einfluss welchen die Umgebung der Pigmente auf die Farbenempfindung auch beim indirecten Sehen hat.

"2. der Umstand, dass die Grösse des farbigen Objectes massgebend ist für die Entfernung vom Centrum, in welcher es noch farbig empfunden wird.

"3. dass Pigmente verschiedener Farbentöne unter sonst gleichen Umständen verschiedene Grenzzonen für die Erkennbarkeit der Farbe zeigen.

"4. dass in die verschiedenen Meridianen der Netzhaut die

<sup>18</sup> Baird, J. op. cit., pp. 12-13.

Grenzzonen für die Farben sehr verschieden weit von dem Fixationspunkte liegen.

"5. Schon Purkinje hat verschiedene Uebergänge durch Farbtöne und Farbennuancen beobachtet, und zwar geht auf schwarzem Grunde nach Aubert: Roth durch Rothgelb und Gelbgrau zu Grau, Blau durch immer weisslichere Nuancen zu Grau, Grün durch Graugelb zu Grau, Gelb durch Graugelb zu Grau.

"6. Donders und Landolt haben nachgewiesen, dass die Farbenempfindung auf den peripherischen Netzhautzonen eine dem Centrum gleiche bleibt, wenn die Intensität der Beleuchtung gesteigert wird: also auch beim indirecten Sehen sind Gesichtswinkel und Helligkeit massgebend für die Farbenperception.

"7. dass die peripherischen Theile der Netzhaut für die Farbenempfindung viel schneller ermüden, also die centralen."<sup>19</sup>

To Baird's fifth point of summary, the closest approximation that the writer is able to find anywhere in Aubert's works is the sixth conclusion quoted above from the *Optik*. This is: "Donders and Landolt haben nachgewiesen, dass die Farbenempfindung auf die peripherischen Netzhautzonen eine dem Centrum gleiche bleibt, wenn die Intensität der Beleuchtung gesteigert wird: also auch beim indirecten Sehen sind Gesichtswinkel und *Helligkeit massgebend für die Farbenperception*. Nagel bestätigt Landolt's Angabe."<sup>20</sup> The question here is whether *Helligkeit* in the above quotation means *brightness of stimulus* which, if our assumption with regard to the source of his authority is correct, Baird has apparently interpreted it to mean. The following points may be cited to show that such an interpretation is very strongly open to question. (a) Aubert himself does not use *Helligkeit* in connection with a qualifying phrase, for example, *Helligkeit der Farben* or its equivalent, while so far as the writer is able to determine, he never uses *Helligkeit* as referring to the *brightness of color* without the qualifying phrase. For example, in his *Physiologische Optik* p. 527 he uses *Helligkeit der Farben*; p. 528, *Helligkeiten der verschiedenen Abtheilungen des Spectrums*; p. 529, *Helligkeiten der Farbtöne des Sonnenspectrums*;

<sup>19</sup> Aubert, H. *Physiologische Optik*, Leipzig, 1876, pp. 541-545.

<sup>20</sup> Aubert, H. *Physiologische Optik*, Leipzig, 1876, p. 545.

same page, Helligkeiten der Farben; p. 530, Helligkeiten der Abtheilungen des Spectrums. In his Physiologie der Netzhaut p. 109 he uses, Helligkeit der Pigmente. The same expression is used again on pp. 111 and 112. (b) When using *Helligkeit* without a qualifying phrase, Aubert commonly refers to the *intensity or brightness of the general illumination*. For examples of this usage, see Physiologie der Netzhaut, pp. 109, 110, 124; Physiologische Optik, p. 532, and other places. (c) No evidence can be obtained from Landolt from whom the citation is made that brightness of color is referred to. In fact the evidence is strongly against this interpretation. As will be shown in detail (pp. 24-25), Landolt worked with colors of varying energy. He used as stimuli very intense spectral light and pigment papers. The energy of the former was varied directly, of the latter by changing the general illumination which altered the amount of colored light reflected to the eye. In the general statement of his problem Landolt is not concerned with the effect of the brightness of his colors; nor are his results couched in terms of the effect of the brightness of colors. His sole interest was to find the effect on sensation of using colors of great energy or intensity. In the above quotation from Aubert, then, we may conclude that it is strongly open to question whether *Helligkeit* is not also used here in the sense in which we have shown that Aubert most frequently uses the term: the brightness of the general illumination (see (b) above). If so, what he really does claim in this statement, therefore, is that when one is working with pigment colors, the degree or brightness of the general illumination with all of its influences, namely, its effect on the intensity of color, on the brightness of color, on the brightness of the surrounding field, the preëxposure, etc., is one of the factors that determine the extent of the color field; not simply one of these influences, the brightness of color, as he is interpreted to claim by Baird.<sup>21</sup> Moreover, the writer is compelled to say that in a careful reading of all the articles by Aubert contained

<sup>21</sup> For Aubert's own statement of his opinion on the question of the influence upon color sensitivity exerted by the brightness of the stimulus see this article, pp. 40-44.

in the long list to which Baird refers, she is unable to find a single statement that would justify the conclusion that Baird has drawn in his fifth point of summary.

Towards Landolt and Abney, Baird takes a slightly different attitude. He does not hold that they have mentioned a need to equate in brightness. In their results, however, he finds justification for equating the white-values of his own stimuli from the construction he puts upon their use of the words *luminosity* and *intensity*. Abney,<sup>22</sup> a physicist, had defined *luminosity* as equivalent to *intensity*. In his experiments he had increased or diminished the luminosity or, as he also says, the intensity of a beam of light by interposing in its path a wedge graduated in thickness. The wedge was made of gelatine in which were scattered black opaque particles. The energy of the beam of light was diminished by amounts depending on the thickness of that part of the wedge through which it was made to pass. This "obstruction method" resulted not only in a decrease of the energy of the light, but in a darkening of the stimulus. But Abney was not at all concerned with the effect of the lightness or darkness aspect of the stimulus:—in other words, with the relative inhibitive action of white and black and its influence on the limits of color sensitivity. His purpose was to vary the energy of the light-waves coming to the eye by "obstructing" them by known amounts, and to ascertain the effect of this change upon the color limits. It is true that the lightness or darkness of the sensation quality was altered incidentally, but he apparently had no thought of saying that this was in any sense responsible for the effect obtained, nor is it a necessary inference from his work. Hence Baird is not justified in stating (p. 31) that Abney makes the brightness of the stimulus a factor in determining the color limit, if we take our clue as to what Baird means by brightness from the following passage. "No determination of the relative extensions of the various color zones can ever yield comparative results unless it be accomplished by means of stimuli of equal brightness, or, more correctly speaking, of equal white-value" (see p. 37). For Abney assuredly, does not mean by *lum-*

<sup>22</sup> Abney, W. op. cit., 155-195.

*inosity* what Baird calls *brightness* or *white-value*. Baird falls into a similar error in his treatment of Landolt's results. He says: "An important feature of Landolt's paper is his insistence that no investigation of color vision is complete unless it takes into account the relative luminosity of the stimuli employed" (p. 17). Now when we read in a footnote (see Baird p. 34) that "Under brightness [of stimulus] is included both absolute and relative luminosity of stimulus, *i.e.*, its own brightness and its brightness contrast with its background," we see that the relative luminosity referred to by Landolt means for Baird relative brightness or white-value, and that the work of Landolt is brought forward as evidence for the necessity of equating the white values of the stimuli. Now Landolt worked with both spectral and pigment stimuli. In case of the first, his method was to increase the energy of spectral light; and in case of the second, to increase the amount of light coming to the eye from a pigment surface by increasing the general illumination of the room. Here, as in the case of Abney, we have a change in the amount or energy of the colored light coming to the eye, and incidentally a change in the white-value of the sensation aroused. No separation is made, however, of the two factors: (*a*) the altered energy of the colored light, and (*b*) the change in the white-value of the sensation aroused. Yet Baird finds reason to conclude from Landolt's results that the white-value of the color influences the limits of sensitivity. It is obvious that Landolt's results do not show this at all. All that they do show is that when the amount of colored light given to the eye is increased or decreased, the extent of the zones of sensitivity is altered.<sup>23</sup>

Whether or not the white-value of the stimulus can be considered to any degree responsible for changes in the color limits

<sup>23</sup> These three cases taken from Baird's discussion of the work of Aubert, Abney, and Landolt are examples of the cases referred to earlier in the chapter in which a confusion as to terminology has led to a wrong interpretation of results which in turn has been the cause of changes in technique. Baird was led by this confusion, in part intrinsic and in part due to his own misinterpretations, to think that these three men considered that the white-value of the colored stimulus affects the extent of the retina's sensitivity to it, and was influenced thereby to equate the white-values of his stimuli without further investigation.

will be considered by the writer in the experimental section of this paper. What we wish to point out here is that without the isolation and the separate investigation of this factor, Baird concludes from the work of previous investigators in which intensity or energy changes have been made in the stimulus, that the white-value of the stimulus influences the boundaries of the color zones and that, therefore, stimuli should be equated in white-value in all work on the limits of color sensitivity. He says: "It has been established in hosts of instances<sup>24</sup> that change of luminosity is, within limits, invariably attended by a corresponding change in the extension of the retinal zone within which the color of the stimulus is recognized. Its significance for the problem is self-evident. No determination of the relative extension of the various color zones can ever yield really comparative results unless it be accomplished by means of stimuli of equal brightness, or, speaking more correctly, of equal white-value" (p. 37). While we grant the significance of changes of luminosity or intensity in the sense in which Abney and Landolt use the terms, we do not admit that this aspect of the stimulus can be standardized in terms of white-value; nor do we grant that any definite evidence whatsoever can be gathered from the results we have quoted above, to show that changes in the white-value of a stimulus affect the limits of the retina's sensitivity to its color, provided the amount of colored light coming to the eye remains unaltered.

To sum up: (a) The white-value of a stimulus may be varied without altering the amount of colored light coming to the eye. This factor, then, may be isolated and its effect on the limits of sensitivity determined apart from any change in the physical intensity of the stimulus. (b) Unless this separation is made, we have no right to conclude that the white-value of the stimulus affects the limits of sensitivity. Baird, for example, drew this conclusion from work in which the separation was not made. (c) The confusion that exists with regard to color terminology

<sup>24</sup> Beside the three instances mentioned here, Baird cites in support of his position the work of Raehlmann, Chodin, Klug, Bull, Hess, and Hegg. How far their work can justly be cited in support of his position will be shown on pp. 51-53.

has been, we believe, in no small measure responsible for Baird's conclusion.

The terminology which we propose to use in this report may be outlined as follows: *Intensity of stimulus* will be used to indicate the energy of light-waves coming to the eye. *Intensity of sensation*, or *apparent intensity*, will be used as its correlative subjective term. So used, it will signify merely energy or voluminousness of sensation and will have no reference whatever to the white-value of a color. *Saturation of the stimulus* will be used to indicate the proportion of colored to colorless light coming to the eye. *Saturation of color* or *saturation of the sensation* will be used as its correlative term and will refer to the proportion of chromatic to achromatic quality in the sensation. The achromatic sensations will be designated by the terms *white*, *black*, and *gray*; and the terms *brilliance* and *white-value* will be used interchangeably to indicate the lightness or darkness of a color.

(b) *The effect of intensity of stimulus.*

A dependence of color sensation upon the intensity of the stimulus has been recognized since the observations of Purkinje. Purkinje noted also that a color stimulus gave a less intense sensation in the peripheral retina than in the central retina. Since that time, it has been claimed (a) that with stimuli of minimal intensity, no color sensation is aroused; (b) that the light-waves arousing the different monochromatic sensations must be of different intensities to give liminal color sensations; that is, the eye is not equally sensitive to waves of different lengths; (c) that, progressively, greater intensity of stimulus is required to give sensation as the stimulus is moved from the fovea to the periphery; (d) that the extent of the color fields is determined within certain limits by the intensity of the stimulus.

The influence of changes in the intensity of the stimulus upon the sensation of color has been investigated by three methods: (1) by determining the effect upon the limens of sensitivity; (2) by determining the effect upon the j.n.d. of sensitivity; (3) by determining the effect upon the limits of sensitivity.

*The effect upon the limens of sensitivity.* When working by this method, the investigator started with a stimulus that was

below the threshold of sensitivity, and increased its intensity until the sensation of color was just noticeable. This increase in intensity was accomplished in three ways: (a) by increasing the illumination of a pigment surface, and consequently the amount of colored light reflected to the eye; (b) by increasing the intensity of the light used to give a spectrum; (c) by increasing the proportion of color in a mixture of colored and gray pigment stimuli.

The first method of increasing the intensity of the stimulus was used in central vision by Purkinje<sup>25</sup> and Aubert.<sup>26</sup> Purkinje observed a representation of the spectrum in pigment colors while daylight advanced. He found that blue was the first color to be seen in its true color tone, green next, yellow next, and red last. He made no measurements, however, of the amount of light required to give a just noticeable sensation. Aubert illuminated a pigment surface 10 mm. square by daylight admitted into a dark-room through an adjustable opening in a window. He found that with an opening  $\frac{1}{4}$ ,  $\frac{1}{2}$ , or 1 cm. square, no sensation of color was obtained. His results are given in the following table.

<i>Opening in window</i>	<i>Stimulus</i>	<i>Sensation</i>
$\frac{1}{4}$ - $\frac{1}{2}$ -1 cm <sup>2</sup>	all	no color
$1\frac{1}{4}$ - $1\frac{1}{2}$	orange	red
2	O, Y, R, rose	O, Y, R, rose
3	blue	blue
3	light green	brown
$3\frac{1}{2}$	light green	light green
5	green	blue
8	green	green

Aubert found that the eye was most sensitive in order to orange, red and yellow, blue, and least sensitive to green.

The second method was used by Raehlmann and Butz. They both used the Bunsen spectroscopic apparatus, which provides for changes in the intensity of the stimulus by means of the Nichol's prism. Raehlmann<sup>27</sup> determined the limens of sensitivity to the

<sup>25</sup> Purkinje, J. op. cit., 1825, II, p. 109.

<sup>26</sup> Aubert, H. Untersuchungen über die Sinnesthätigkeit der Netzhaut. Pogg. Annal., 1862, CXV, pp. 87-116.

<sup>27</sup> Raehlmann, E. Ueber Schwellenwerte der verschiedenen Spectralfarben an verschiedenen Stellen der Netzhaut. A. f. O., 1874, XX, pp. 232-254.

different spectral colors at the center of the retina, and at  $30^\circ$  and  $60^\circ$  in the horizontal nasal meridian. He found that the center was most sensitive in order to green, yellow, blue, violet, and red; and the periphery to yellow, blue, green, violet, and red. Butz's<sup>28</sup> procedure was as follows: He first determined the liminal value for each of his colors at the center. Starting with this value as unit, he determined how much this value had to be altered to give liminal sensation at  $30^\circ$  and at  $60^\circ$  in the horizontal nasal meridian. He found (a) that the sensitivity to each color increases from  $0^\circ$  to  $30^\circ$  and decreases from  $30^\circ$  to  $60^\circ$ ; (b) that the amount of increase in sensitivity from  $0^\circ$  to  $30^\circ$  and the amount of decrease from  $30^\circ$  to  $60^\circ$  is different for the different colors, that is, the ratio of liminal sensitivity to any two colors is not the same from center to periphery; and (c) that the amount of the increase is greatest, and of the decrease is less in order for violet, yellow, blue, green, and red.

The third method was used by Aubert and Chodin, both of whom employed the Masson disc to find the limen of color sensitivity. Aubert<sup>29</sup> found that at the fovea, the eye is more sensitive to orange and yellow than to red and blue. Chodin<sup>30</sup> found the sensitivity in the central retina to be greatest in order for orange, yellow, green, and least for blue. In the periphery, he found that the retina is more sensitive to blue and yellow than to red and green.

(2) *The effect upon the j. n. d. of sensitivity.* The effect of intensity upon the j. n. d. of sensitivity has been investigated by Lamansky and Dobrowolsky. Lamansky<sup>31</sup> used polarized spectral light, and worked in central vision. He found (a) that the j. n. d. of intensity for the different colors increases or, in other words, the sensitivity decreases, as the intensity of the stimulus is

<sup>28</sup> Butz, R. Vorläufige Mittheilungen über Untersuchungen der physiologischen Functionen der Peripherie der Netzhaut. Archiv für Anatomie und Physiologie, 1881, pp. 437-445.

<sup>29</sup> Aubert, H., Physiologie der Netzhaut. p. 136.

<sup>30</sup> Chodin, A. Ueber die Empfindlichkeit für Farben in der Peripherie der Netzhaut. A. f. O., 1877, XXIII,<sub>21</sub>, pp. 177-208.

<sup>31</sup> Lamansky, S. Ueber die Grenzen der Empfindlichkeit des Auges für Spectralfarben. Pogg. Annal., 1871, CXLIII., pp. 633-643.

decreased, and (b) that the j. n. d. of intensity is smallest, or the sensitivity is greatest in order for yellow and green, blue, and red. Dobrowolsky<sup>32</sup> in 1876 worked with the colors of the spectrum in central vision and at various points in peripheral vision. Employing standard and comparison fields, he altered the intensity of the comparison by rotating a Nichol's prism before the light-source until its intensity was just noticeably different from that of the standard. Considering that sensitivity varies inversely as the magnitude of the j. n. d., he found (a) that the sensitivity to the different colors decreases with increase of excentricity; and (b) that the comparative sensitivity for the different colors is the same in the center and in the periphery, that is, the order of sensitivity from greatest to least is in each case, blue, green, red. Later in 1881<sup>33</sup> he worked at seventeen different points in the intensity scale. He found again that the j. n. d. for blue is smallest, for green next, and for red largest. In addition he found that the ratio of sensitivity between any two colors as measured by the j. n. d. is not the same for different points in the intensity scale.

In regard to the methods used in the investigations reported above, it may be noted that in no one of the cases have the intensities of the stimuli used been measured and standardized. Tests of the comparative sensitivity of different parts of the retina to the same color and to different colors may be made with propriety by such methods, but not of a given part of the retina to the different colors. Conclusions can not, then, be drawn by the preceding investigators with regard to the comparative sensitivity either of the center or of the periphery of the retina to the different colors. They can, however, show that the center has not the same comparative sensitivity to the different colors that the periphery has. This conclusion may in fact be drawn from the results of all except Dobrowolsky. Dobrowolsky's results alone show that the center and periphery have the same relative sensitivity for all of

<sup>32</sup> Dobrowolsky, W. Ueber die Empfindlichkeit des Auges gegen die Lichtintensität der Farben im Centrum und auf der Peripherie der Netzhaut. Pflüger's Archiv., 1876, XII, pp. 441-471.

<sup>33</sup> Dobrowolsky, W. Ueber die Veränderung der Empfindlichkeit des Auges gegen Spectralfarben bei wechselnder Lichtstärke derselben. Pflüger's Archiv., 1881, pp. 189-202.

the colors with which he has worked. A fair test of the comparative sensitivity of the eye to the different colors demands either that stimuli of equal energy be used or that the sensitivity be estimated in terms of units that can be compared. So far as the writer knows, no test of the sensitivity of the retina to color has ever been made with stimuli representing equal amounts of energy. Langley (1889) worked with stimuli of equal energy, but his test was for visual acuity.<sup>34</sup> Until stimuli of equal energy are used, it will remain an open question whether or not the retina, either in the center or in the periphery, possesses a different degree of sensitivity to each of the colors.

(3) *The effect upon the limits of sensitivity.* That change in the intensity of the stimulus has an effect upon the limits of sensitivity, has been shown by Abney<sup>35</sup> and others. Abney carried on an elaborate series of experiments with spectral light to show the effect of changes of intensity upon the extent of the color fields. He decreased the intensity of the stimulus by placing before it a gelatine wedge in the form of an annulus or ring. This annular wedge was one inch broad. It was graduated in thickness and its transparency was further regulated by black opaque particles which had been mixed with the gelatine in its semi-fluid state. The value of light admitted at O or at the thinnest part of the ring, was 10,000 units;<sup>36</sup> that admitted at  $360^\circ$  was 8 units. By interposing the annular wedge in a plane perpendicular to the path of the light and producing the proper amount of rotation, the intensity of the stimulus was reduced by graded amounts. Abney concluded that there is a simple relation between the intensity of the stimulus and the size of the color field.

The extreme position with regard to the effect of intensity upon the extent of the color field is taken by Landolt in the following passage. "In ein absolut dunkles Zimmer fiel nur durch eine kleine Öffnung im Fensterladen directes Sonnenlicht. Dieses wurde auf das äusserste Ende des Perimeterbogens gelenkt. Während wir unser Auge ins Centrum des Bogens setzen, bracht man in die

<sup>34</sup> For discussion of Langley's work, see this paper p. 71.

<sup>35</sup> Abney, W. op. cit., pp. 155-195.

<sup>36</sup> No statement of the value of these units is made by Abney.

kleine, intensive beleuchtete Stelle farbige Papiere von möglicher Intensität der Färbung. Nun bewegt sich das Auge langsam vom entgegengesetzten Ende des Bogens nach Scheitelpunkte zu und es zeigte sich dabei, dass wenigstens mit der innern Netzhautpartie alle Farben schon bei  $90^\circ$  erkannt wurden. Die Grösse des Objectes betrug weniger als  $1 \text{ cm}^2$ .

“Als dieselben Prüfungen auch mit Spectralfarben zu machen, entwarfen wir ein Sonnenspectrum im sonst dunkeln Zimmer und liessen es durch eine achromatische Linse auf einen Ende des Perimeters befindlichen Schirm fallen. Dieser hatte eine veränderliche Spalte, mittelst welcher man die einzelnen Farben aus dem Spectrum isolieren konnte. Während wir nun wiederum nach langer Adaptation, und bei verbundenem zweiten Auge das eine Ende des Bogens fixierten, würde von einem Assistenten irgendeine Farbe des Spectrums auf die Spalte gelenkt, und wir drehten nun, unter stehender Fixation unserer Fingerspitze, welche sich auf dem Bogen bewegte, das Auge allmählig der Farbe entgegen. Es zeigte sich auch hier wiederum dass alle Farbe schon bei  $90^\circ$  erkannt werden, wenn sie intensiv genug sind.”<sup>37</sup>

The first to recognize the need for making any sort of intensity equation of the stimuli used to investigate the relative sensitivity of the retina to the different colors was Ole Bull.<sup>38</sup> His purpose was to find an accurate method for investigating and measuring the sensitivity of the retina to the different colors. The first essential condition of the method, he considered, must be to equate the colors in saturation and brightness. Briefly, his method of equating in saturation consisted of using complementary colors of such relative intensity that they cancelled each other in a 1:1

<sup>37</sup> Landolt und Snellen. *Ophthalmometrologie*. Handbuch der ges. Augenheilk. von Graefe und Saemische, 1874, III., p. 70. The above quotation is given in full in the original in order to confirm (a) the statement made p. 16 concerning the interpretation of Aubert's statement of Landolt's results, and (b) the writer's interpretation, as opposed to Baird's, of Landolt's results, stated p. 18.

A brief summary of the above work is given by Landolt in *Klinische Monatsblätter für Augenheilkunde*, 1873, XI., pp. 376-377; and in *Annales d'Oculistique*, 1874, LXXI., pp. 44-46.

<sup>38</sup> Bull, O. *Studien über Lichtsinn und Farbensinn*. A. f. O. 1881, XXVII, pp. 54-154.

ratio; that is, he used colors whose color-cancelling or color-quenching power was equal. It will be shown later in the paper (pp. 63-69), that this method is an anomaly, and, so far as the writer knows, is not justified in any investigation of color sensitivity that has yet been proposed. It certainly does not warrant conclusions concerning the relative limits of color nor the relative sensitivity of the retina to the different colors. Using this method of equating Bull concludes, however, that the retina, central and peripheral, is most sensitive to blue, then to yellow, then to red and green.

The second to recognize this essential condition was Hess<sup>39</sup> who made an exhaustive "Prüfung des Farbensinnes auf der peripherischen Netzhaut." Hess's object was primarily to furnish Hering with experimental evidence that would enable him to refute the Young-Helmholtz theory as modified by Fick to explain color-blindness. Young's view that congenital color-blindness is due to the absence of one of the three kinds of nerve fibres conceived by him to exist in the retina was adopted by Maxwell and Helmholtz, and extended by the latter<sup>40</sup> to explain the so-called peripheral color-blindness of the normal eye. Helmholtz believed that the peripheral retina is red-blind, and that this fact could be explained by assuming the absence of the red-sensing fibre. In 1873 Fick<sup>41</sup> and Leber<sup>42</sup> independently pointed out that this explanation of the peripheral color-blindness is inconsistent with the fundamental assumptions of the Young-Helmholtz theory. Fick declared that according to this theory the sensation of white can be aroused only by the stimulation of all three fibres in equal amounts, or to express it in another way, in balanced proportions. Now, if one fibre were inactive in the

<sup>39</sup> Hess, C. Ueber den Farbensinn bei indirectem Sehen. A. f. O., 1889, XXXV, pp. 1-62.

<sup>40</sup> Helmholtz, H. Handbuch der physiologischen Optik. 1st ed., 1867, pp. 301, 845.

<sup>41</sup> Fick, A. Zur Theorie der Farbenblindheit. Arbeiten aus dem physiol. Laborat. der Würzburger Hochschule, pp. 213-217.

<sup>42</sup> Leber, T. Ueber die Theorie des Farbenblindheit und über die Art und Weise, wie gewisse, der Untersuchung von Farbenblinden entnommene Einwände gegen die Young-Helmholtz'sche Theorie such mit derselben vereinigen lassen. Klin. Monatsblätter f. Augenheilk., 1873, XI, pp. 467-473.

peripheral retina, the sensation of white could never be produced in that part of the retina; but instead, the sensation proper to the combined action of the other two fibres would be aroused by white light. If, for example, in the middle region of the retina, the red-sensing fibre, and in the more peripheral regions both the red- and the green-sensing fibres were lacking, the sensation produced by white light in the former case would be blue-green, in the latter blue.<sup>43</sup> Further, if one or more fibres were absent in the outer zones of the retina, all the color sensations in these regions would be more saturated than those in the more central regions. For when all three fibres are present, as in the central retina, red, green, or violet light, for example, will stimulate not only its own proper fibre strongly, but will also stimulate the other two weakly. A certain amount of this total stimulation will be in the proportion to give the sensation of white, or more properly speaking, the sensation of gray, and the effect of this colorless component will be to reduce the saturation of the color sensation aroused. If, then, in the peripheral retina only one or two fibres are present, the colorless component will not be present to reduce the saturation of the color sensation, hence, other things being equal, the sensation of color should be more saturated here than that given in the central retina. Fick proposed the following modification of the theory to account for the color phenomena of the peripheral retina. He assumed that from the middle toward the periphery of the retina the relative excitability of the three nerve fibres to lights of the various wave-lengths constantly alters in such a way that at a certain distance from the fovea, namely, in the zone called by Helmholtz red-blind, the red-sensing fibres possess the same excitability as the green-sensing fibres toward lights of all wave-lengths;

<sup>43</sup> While in the opinion of the writer, Fick is correct in saying that white could not be produced in the extreme periphery by the action of two of the retinal fibres he is not right in saying what would be produced. For by a literal interpretation of the curves of excitation drawn by Helmholtz to represent his theory, none of the color experiences can be produced by the action of two fibres, excepting those in a small region of the spectrum in the violet. All other color experiences are produced by the combined excitation of all three fibres in some proportion.

and that further toward the extreme periphery, all difference between the relative excitability of the three fibres diminishes and finally disappears. In the red-blind zone, then, the intensity-curves for the red- and green-sensing fibres coincide, and in the totally color-blind zone, the curves for all three coincide. Curves drawn in accord with these assumptions will, it is contended by Fick, explain the types of color-blindness found in the peripheral retina without violating any of the fundamental principles of the Young-Helmholtz theory. Helmholtz accepts the essential points of this modification and incorporates them in his theory in his later edition of the *Physiologische Optik*.<sup>44</sup>

In order to disprove Fick's assumption that the relative intensity of response of the three fibres varies from center to periphery of the retina, and thus discredit the Helmholtz theory, Hess advanced three lines of argument. These arguments are as follows. In the first place he claims that there are three colors of the spectrum, a yellow, a green, and a blue, and a mixed color, a bluish-red, which are all invariable in tone from the center to the periphery of the retina.<sup>45</sup> In the second place, he shows that the proportions in which the complementary colors combine to produce white do not change for the different parts of the extramacular retina. And in the third place he attempts to show that a constant ratio of sensitivity to the members of each pair of complementary colors obtains throughout the retina. In order to make his third point he attempted to obtain red and green stimuli such that the red *Valenz* of the one, or, as he defines *Valenz*, its capacity to arouse red sensation,<sup>46</sup> should equal the

<sup>44</sup> Helmholtz, H. *Handbuch der Physiologischen Optik*. 2nd ed., 1896, p. 373.

<sup>45</sup> For a refutation of this point see footnote, p. 85.

<sup>46</sup> Hess defines the *Valenz* of his stimuli as their power to arouse color sensation. The writer would question this use of the term. According to its accepted chemical usage, the term might be applied with some degree of propriety to the power which, in terms of the Hering theory, a color possesses to combine with or cancel its complementary color, but scarcely to its power to arouse sensation. Hess, we presume, applies this term to the power of a color to arouse sensation because he assumes that this power is the same or at least equivalent to its power to cancel the complementary color. But since, as we shall show later (see p. 65), this assumption is far from correct, we strongly question the propriety of calling the power of a color to arouse sensation its *Valenz*.

green *Valenz* of the other. He describes his method of doing this as follows: "Als das Nächstliegende erscheint es nun, einen roth- und einen grün-wirkenden Pigmente den gleichen Roth- und Grünwerth dann zuzuschreiben wenn dieselben zu gleiche Theilen, z.B. auf dem Kreisel gemischt eine farblose Mischung geben" (p. 39). The same procedure was used in obtaining his blue and yellow stimuli. Using as stimuli, then, a red equal in cancelling power to a green, and a blue to a yellow, he determines the limits of sensitivity to these four colors. He finds that the limits for his red stimulus coincide with the limits for his green, and the limits for his blue with the limits for his yellow. The limits for blue and yellow, however, fall further out from the fovea than they do for red and green. From these results he concludes (*a*) the sensitivity of the retina from center to periphery falls off with the same rapidity for red as for green, and for blue as for yellow: and (*b*) the sensitivity for both red and green falls off more rapidly than for blue and yellow. Among his conclusions one also finds: "Bei den von uns mitgetheilten Untersuchungen ist die Prüfung des Farbensinnes auf der peripheren Netzhaut zum ersten Male mit genauer Berücksichtigung aller jener Bedingungen vorgenommen worden, welche unverlässlich sind wenn die mit verschiedenen Farben gewonnenen Resultate untereinander verglichen werden sollen" (p. 56). These conclusions are open to the following criticisms: (1) His results do not warrant the statement that the sensitivity for red falls off as rapidly as for green, and for blue as for yellow. For this statement is based on the assumption that if in passing from center to periphery, sensitivity ends at the same point on the retina for two stimuli which have equal power to arouse sensation at the center, they must still have equal power to arouse sensation at the periphery, that is, sensitivity has fallen off as rapidly for one as it has for the other. Now in the first place this assumption begins with a fallacy. For his stimuli were not equated in power to arouse sensation but in cancelling power, and, as we shall show later, the power of a color to arouse sensation and its power to cancel its complementary color are not at all equivalent. And in the second place the assumption is itself incorrect.

for, because of the abrupt decrease in sensitivity as the limits are approached, the relative sensitivity to the two colors may have changed greatly and still their limits have coincided. We have found for example that working with colors of normal saturation under good illumination, it takes, varying with the color, a difference of  $90^\circ$  to  $120^\circ$  of color to make a difference of  $1^\circ$  in the limits. Hess should have determined the limens of color at various points from the center to the periphery of the retina, and have found out whether the ratios of the liminal values of his pairs of stimuli were equal at all of these points.<sup>47</sup> If so, the sensitivity to each member of the pair must have fallen off with equal rapidity from point to point, otherwise a change of ratio would have occurred. This method would have had the following advantages. (a) Account would have been taken of sensitivity at a large number of points from fovea to limits. (b) Much smaller changes in sensitivity would affect the limens than would affect the limits, especially until very near the limits. (c) No equation of stimuli with its attendant disadvantages would have been needed as long as the liminal values for each color were obtained in terms of the same stimulus all the way out. (2) But even if an equation made in terms of cancelling power were the equivalent of an equation made in terms of the power to arouse sensation, he would not have been justified in concluding, so far as his method of working is concerned, that the red-green sense decreases more rapidly than the blue-yellow sense, because this method afforded him no means of equating the intensities of the members of one of the pairs with those of the other pair. (3) Nor is he justified in his claim that he is the first to investigate the color sensitivity of the peripheral retina who has paid due regard to all the conditions which are essential if the results obtained for this sensitivity are to be compared with one another. One scarcely knows where to begin to refute a

<sup>43</sup> While in the opinion of the writer, Fick is correct in saying that white just noticeable difference of sensation could have been determined at each of these points in the retina for various points in the intensity scale. An account could thus have been had of the retina's sensitivity to the members of the pairs of colors at as many degrees of intensity of sensation as was desired.

statement so broadly overdrawn as this. The inadequacy of his treatment of the factors: general illumination, brightness of the stimulus, brightness of the surrounding field and preëxposure, is obvious to anyone who knows the effect of these factors. This inadequacy, however, will be noted at various other points in the paper. It will be sufficient at this point to consider only his handling of the factor, intensity, and that only briefly, for it will also be discussed in some detail at another point in the paper. Hess's problem was narrow and happens to furnish one of the very rare cases in which a subjective equation of the intensity of the stimuli used is justified. But his conclusion with regard to his method of handling the intensity factor is broad and specifically refers to all investigations of sensitivity in which results are to be compared. Such a conclusion is most assuredly not justified. In fact one might almost say that the converse of his conclusion is true. In no investigation of the comparative sensitivity of the retina to the different colors where absolute values are wanted is a subjective equation permissible. Such an equation begs the question at the outset. In such an investigation when an equation is needed, it should be made in terms of a common objective unit, for example, the unit of energy, and when an equation is not needed, the sensitivity should be estimated and expressed in terms of this common objective unit, or some other unit in terms of which results can be compared. In no case, so far as the writer is at present able to outline the field, is a subjective equation justified in an investigation of sensitivity except in certain problems relating to existing color theories or assumptions made for systematic purposes. And not even in these problems, so far as the writer is familiar with them, is an equation made in terms of cancelling power justified in an investigation of sensitivity.

Hegg<sup>48</sup> was the third investigator to attempt intensity equation. He posits three conditions to be fulfilled "*für die Untersuchung des peripheren Farbensinnes.*" (a) The colors must be physiologically pure, that is, each color must be sensed similarly in all parts of the retina sensitive to it. (b) The colors must be

<sup>48</sup>Hegg, E. *Zur Farbenperimetrie.* A.f.O., 1892. XXXVIII., pp. 145-168.

of equal value in regard to brightness. (c) They must be equal in regard to color content (farbigen Gehalt). Concerning the third condition, Hegg writes: "Wenn es sich nun, was von theoretischen und praktischen Gesichtspunkten aus betrachtet von gleich grosser Wichtigkeit ist, darum handelt, die verschiedenen Farbempfindungen mit einander zu vergleichen, die physiologische Erregbarkeit entsprechender Nerven-elemente nach einem gemeinsamen Massstab zu messen, so ist es selbstverständlich unumgänglich nothwendig, mit gleichgemessenen Reizen die Versuche anzustellen und unsere definirbaren, invariablen Farben anzupassen" (pp. 148-149). Hegg's "gleichgemessene Reizen" were obtained according to the method first used by Bull and endorsed by Hess. But this method of equating can not warrant any conclusion concerning the relative limits of peripheral sensitivity to the different colors. Hegg is then not justified in concluding "dass die Grenzen für Roth und Grün zusammenfallen. Die Grenze für Gelb sind durchwegs ca.  $1^{\circ}$  enger als für Blau, vielleicht wegen der stärkerem Brechung der blauen Strahlen?" (p. 166).

Baird<sup>49</sup> was the next to use this method of equating. He determined the relative extension of the different color zones, employing as stimuli light transmitted through gelatine filters. Like his predecessors he also concludes beyond what is justified by his method of working. He expresses his results as follows: "The zone of stable red is coextensive with that of stable green; the zone of stable yellow is coextensive with that of stable blue; and the yellow-blue zone is much more widely extended in all directions than is the red-green zone" (p. 61).<sup>50</sup>

<sup>49</sup> Baird. loc. cit.

<sup>50</sup> Fernald (Psychol. Rev. Monog., 1909, X, pp. 60-67) as a minor point in her study of the color sensitivity of the peripheral retina, made a hurried investigation in the nasal and temporal meridians of the limits of an Urroth and an Urgrün, an Urgelb and an Urblau that she claimed were of equal saturation. Her method of equating these stimuli was the same as that of Bull, Hess, Hegg, and Baird. The limits of these stimuli were, she states, determined hurriedly, most of the observations being made at intervals of  $5^{\circ}$  on the peripheral retina; for example, she states that Urgelb and Urblau were both seen at  $85^{\circ}$ , and not seen at  $90^{\circ}$ . In spite of the looseness of these determinations, she concludes that "the limits for the Urgrün are practically coextensive with those for the Urroth, and the fields for the

Bull, Hess, Hegg, and Baird all claim, then, that when the investigation is made with stimuli equated in brightness and in terms of cancelling power, the limits for *Urroth* and *Urgrün*, for *Urgelb* and *Urblau* coincide. It may be inferred from their work that they believe that at least one of the reasons for the non-coincidence of limits obtained by previous investigators is that the colors used were not equated in intensity. Evidence, however, can be derived from the work of Kirschmann,<sup>51</sup> 1893, that this can not at least be considered the sole reason. In Kirschmann's case, in fact, it apparently can not be considered as having any influence at all in producing the non-coincidence obtained. Although his pairs of colors were not equated in intensity, it is obvious that the deviations he obtained from coincidence of limits can not be explained as due to differences in intensity between his stimuli. Kirschmann mapped many meridians of the retina for the limits of sensitivity with both spectral and pigment stimuli. The pairs of colors were not equated in intensity. His color maps show that the outline of the field for each color is irregular in the different meridians, and is different from that of any other color. In general the field for blue is wider than that for yellow, but in certain meridians this order is reversed. The red field is generally wider than the green, but in some meridians the green is the same or wider than the red. Further, the difference between the limits of the colors in some meridians is considerably greater than in others. It is evident *Urgelb* with those for the *Urblau*" (p. 65). In addition to applying to this work the criticisms passed above concerning the more careful work of Bull, Hegg, and Baird, all of whom drew conclusions similar to Fernald's, one may express surprise that work so sketchy should be considered as warranting any conclusion whatever. Fernald states, however, that differences of 5° in limits are too small to be of any significance (p. 66). She makes this statement apparently because with the varying conditions of illumination under which she worked, 5° seem to be a normal variation in limits. (It may be too that she considers that this gives her warrant for working only at intervals of 5°.) But as will be shown in the experimental section of this paper, a difference of 5° in limits represents a difference in sensitivity sufficient to raise the limen of sensitivity 200°. In more careful work, then, limits which varied 5° would hardly be considered as "practically coextensive."

<sup>51</sup> Kirschmann, A. *Die Farbenempfindung bei indirectem Sehen*. Philos. Studien, 1893, VIII., pp. 562-614.

that irregularities of this kind can not be due to a difference in the intensity of the stimuli employed, for if blue and yellow, for example, have the same limit when equated in intensity, their limits should retain the same general outline when the colors are of unequal intensity, and should differ only in their distance from the fovea. The zone for the more intense color should in every meridian be regularly some degrees wider than that for the less intense. Relative to the issue between Kirschmann and Bull, Hess, Hegg, and Baird, it is interesting to find that Bull, Hess, and Baird, who all claimed to find coincident limits in all parts of the retina for the paired colors obtained results which, when examined in detail, show the same deviations from coincidence as those which Kirschmann found. Baird, for example, determined the limits of the four principal colors in eight meridians, and concludes: "The results show that the zone of stable-red is coextensive with that of stable-green; that the zone of stable-yellow is coextensive with that of stable-blue."<sup>52</sup> An inspection of his table of results shows however, that this coincidence is extremely rough. In the results for every observer it is found that in some meridians the green field is wider than the red by  $1^\circ$ ,  $2^\circ$ , or  $3^\circ$ ; in other meridians, there is coincidence of limits; and in still other meridians, the green field is narrower than the red by  $1^\circ$ ,  $2^\circ$ , or  $3^\circ$ . The same thing is true of blue and yellow. In general, but not in every meridian, yellow seems to be wider than blue on the nasal retina, blue wider than yellow on the temporal. Hess's and Bull's results show similar variations which are in some cases of even greater extent. It is evident, however, from their conclusion concerning the coincidence of limits that they regard these variations as insignificant, probably no more than their normal M.V. for the rough conditions under which they worked. But it should be borne in mind that  $2^\circ$  or  $3^\circ$  of difference in limits is not insignificant when conclusions are to be drawn from the results with regard to the relative sensitivity of the peripheral retina to the members of the pairs of complementary colors. Because of the abrupt falling off in sensitivity just before the limit is reached (see p. 117 ff.), a difference of

<sup>52</sup> Baird, J. op. cit., p. 61.

$2^{\circ}$  or  $3^{\circ}$  in the limits represents quite a large difference in sensitivity. For example, according to our results a difference of  $2^{\circ}$  in limits represents a difference in sensitivity sufficient to raise the limen for yellow  $120^{\circ}$ , for green  $100^{\circ}$ , for red  $160^{\circ}$ , for blue  $170^{\circ}$ ; and a difference of  $3^{\circ}$  represents sufficient to raise the limen for yellow  $210^{\circ}$ , for green  $215^{\circ}$ , for red  $210^{\circ}$ , for blue  $215^{\circ}$ . Obviously the point is too important to be passed over without re-examination by better methods of working. The limits should be re-determined under conditions that do not give so large an M.V. This has been done by the writer with such a control of all the factors that cause variable results that her M.V. from observation to observation is less than  $1^{\circ}$ . The results obtained show that this difference in limits is real, and not an error due to any inaccuracy in the method of working. In some meridians the limits coincide, in others they diverge. In short, when the zones are outlined by lines connecting the points representing the limits in the different meridians, these lines for the pairs of colors do not coincide, but criss-cross in a very irregular manner. Therefore, on the basis of our own results as well as those of Kirschmann, the inference can not be drawn from the work of the men who have equated their stimuli in terms of cancelling power that coincidence of limits had not been obtained up to that time because stimuli so equated had not been used. Nor can the conclusion be drawn that even if the stimuli had been properly equated, coincidence of limits would have been obtained. In fact the converse of this conclusion can apparently be drawn.<sup>53</sup>

<sup>53</sup> The writer used as stimuli in these experiments the standard yellow, red, green, and blue of the Hering series. She did not use colors stable in tone as Bull, Hess, Hegg, and Baird claimed to use, because an exhaustive study of this question showed her that stability of tone for all meridians of the retina can be obtained for blue alone of the four principal colors. For a further statement in support of this point, see footnote p. 85. In any event, she is unable to see how the fact that the red and green of this series appear yellow in a small region of the peripheral retina could have any effect on the coincidence of limits for red and green, unless the limit for red, for example, is taken as the point at which all sensation of color disappears, regardless of whether this color is red or yellow. (The writer considered in her own experiments that the limit for a color was the point at which the color sensation lost all trace of its original quality.) However, even if the point at which all sensation of color whatever disappeared be

Therefore, the argument based upon it against Fick's modification of the Helmholtz theory to explain the color-blindness of the peripheral retina can also be refuted insofar as the results on the coincidence of limits can be considered as furnishing argument. This argument is, it will be remembered, that the sensitivity of the retina from fovea to periphery falls off as rapidly to one of the members of the pairs of complementary colors as to the other. It should be borne in mind, however, that the results needed in order to make this argument can not be obtained by a determination of the comparative limits of sensitivity alone.

considered the limit, the following consideration shows that the small component of yellow present in the peripheral retina in the sensations aroused by the red and the green of this series could not have caused the criss-crossing of limits any more than a difference in intensity could have caused it. In fact the point in reality reduces to a question of intensity. If, for example, one of the colors were stronger than the other, this color would have relatively more power to arouse this yellow component, than it would if the colors were of equal intensity. That is, the zone through which the yellow component would be sensed would be broader for this color than it would have been had both colors been of the intensity of the weaker color. And relative to the zone in which a yellow component was sensed, it would not be irregularly broader in some meridians for the stronger color and in other meridians for the weaker color, for in regions in which the yellow sense is relatively weak the zone would narrow for both stimuli. In short, while the influence of the yellow component might cause the zone of sensitivity to broaden for one member of the pairs of stimuli as compared with the other member, it could not cause it to become alternately broader and narrower, in other words, to criss-cross. It is just as obvious that difference in brightness can not be offered as an explanation of this criss-crossing, even if difference in brightness could be shown to have an effect on the limits of sensitivity to the pairs of colors in question. But, as we will show in the experimental section, difference in brightness can be considered as having no effect whatever on the limits of sensitivity. Hence on both counts, difference in brightness can be ruled out of consideration. It would seem, then, that we can conclude that the criss-crossing of limits represents a real relation of sensitivity to the members of the pairs of colors in the far periphery of the retina, even before the investigation is made with stimuli properly equated in intensity. Moreover, when we take into account the fact that a difference of  $2^{\circ}$  to  $3^{\circ}$  in the limits represents a large difference in sensitivity we have considerable reason for believing that the ratio of the sensitivity to one member of a pair of complementary colors to the sensitivity to the other member is not the same in all parts of the retina. The point will be definitely determined in the near future by the writer by a careful determination of limens from point to point in many meridians of the retina, with stimuli which consist of lights of spectral purity, measured in terms of a common unit of intensity.

Knowledge is needed of the comparative sensitivity all the way out. Moreover, this knowledge must be based on actual determinations at points not widely separated from each other. The limit is only one of the points at which determinations should be made. In fact, it can scarcely be said to sustain any more important relation to the problem than any other point far removed from the fovea. Furthermore, conclusions can not be drawn from work done by the method of limits with regard to the comparative falling off in sensitivity from fovea to periphery unless it has previously been inferred from a comparison of the results at the center with the results at the limits what the comparative sensitivity should be at the points intervening, as was done by Hess. But this is wholly unjustifiable, for if there is one principle above another that the determinations of sensitivity in the peripheral retina bring out, it is that inferences can not be drawn about sensitivity between points at all widely separated. In fact, practically no conclusions of systematic importance can be justified at all from results obtained by the method of limits, although conclusions of great importance to theory have frequently been drawn from such results. The method of limens should be used instead, for by means of it accurate account can be taken of sensitivity at any point that is desired.<sup>54</sup>

A brief survey of our discussion of intensity shows the following facts:

1. The intensity of the stimulus is a factor influencing both the limens and the limits of color sensitivity. It should, therefore, be carefully standardized in all determinations of sensitivity.

2. The conclusions that have been drawn up to this time concerning the comparative sensitivity of the retina to the different colors have not been justified because of the methods of working that were used in the investigations from the results of which they were drawn; for (*a*) either no standardization of the intensity of the stimuli used had been made; or (*b*) this standardization had been made by an improper method. From the results of

<sup>54</sup> As stated in footnote p. 83, if a more exhaustive study of the point is wanted, the results of the method of limens should be supplemented by a determination of the just noticeable difference in sensation for various points in the intensity scale at each point of the retina investigated.

the work as it was done the following conclusions alone can be drawn: (a) The comparative sensitivity to the different colors is not the same at the center as it is in the periphery of the retina. This conclusion may be drawn from the results of Chodin and Raehlmann and Butz for the limens of sensitivity. And (b) it is not the same for the members of the pairs of complementary colors at all points even in the small region of the peripheral retina that has been examined, namely in the region comprehended by the criss-crossing limits for these colors. This conclusion may be drawn from Kirschmann's results, from our own, also from those of Bull, Hess, and Baird as shown in their tables giving the limits of their stimuli in different meridians.

3. Broader conclusions than this are justified only when the comparative sensitivity of the retina to the different colors is determined with stimuli properly standardized in intensity. As has been briefly pointed out, the comparative limits of sensitivity can be determined only when the intensities of the stimuli used have been standardized in terms of a common objective unit, for example, the unit of energy; and the comparative limens only when the intensities of the stimuli used have been estimated in terms of this common objective unit, or some other unit in terms of which results can be compared.<sup>55</sup> This point will be discussed more fully later in the paper (see p. 63 ff.).

(c) *The effect of brightness of the stimulus.*

There has been very little discussion by previous investigators whether the brightness or white-value of a color affects the retinal limits of sensitivity to that color, and whether this aspect of the stimulus need then be taken into account in determining the relative extent of the color zones. We have already shown that in such work as there has been, the brightness factor has been obscured by and confounded with the intensity factor.

The confusion of the intensity and brightness aspects of color was noted by Langley in an article entitled *Energy and Vision*, *Philos. Mag.*, 1889, XXVII, 5th ser., p. I. Langley writes: "While it is quite a familiar fact that the luminosity of any spectral ray increases proportionately to the heat in

<sup>55</sup> It is scarcely needful to point out that the same kind of standardization is required in order to determine the comparative j. n. d's. for the different colors.

this ray, and indeed is but another manifestation of the same energy, I have recently had occasion to notice that there is, on the part of some physicists, a failure to recognize how totally different optical effects may be produced by one and the same amount of energy according to the wave-length in which this energy is exhibited. I should not perhaps have thought it advisable to make this last remark, were it not that there has appeared in a recent number of *Wiedemann's Annalen* a paper by H. F. Weber on "*The Emission of Light*", in which he tacitly makes the assumption that the luminosity of a color is proportionate to the energy which produces it, an assumption which it is surprising to find in a paper of such general merit and interest."

This confusion, insofar as it has not been due to a misinterpretation of terms, seems to have arisen because the usual method of varying the one factor has necessitated an accompanying variation of the other. Further, the fact that when the intensity or energy of spectral light is increased to a maximum, the color is lightened until white is produced,<sup>56</sup> has been responsible for the view that light colors are more intense than dark colors, and has led to the custom of determining the energy or intensity of colored light by photometric methods. The photometric method can not, however, be used directly for estimating the intensity of colored light for two reasons. (a) Direct radiometric measurements of energy show that the relative values of the colors of the spectrum as determined by the two methods do not at all coincide. The photometric curve, for example, of the spectra of all light sources of normal intensity is highest in the yellow-green and lowest in the blue and red. The radiometric curve of the visible spectrum of sunlight of the same intensity is, on the other hand, according to Langley,<sup>57</sup> highest in the red near the C line and lowest in the violet; while the radiometric curves of the visible spectra of most of the artificial sources of light, such as the Nernst, tungsten, and arc lights, are highest in the extreme red and lowest in the violet. (b) The relative photometric values of the colors of all spectra differ widely for different intensities of the same light-

"This statement is true only if the original color has maximal saturation. When one increases the intensity of colors whose original intensity is slight, red and yellow become lighter, blue and green darker. These brightness changes are known as the Purkinje phenomenon.

<sup>57</sup> Langley, *Energy and Vision*. Amer. Journ. of Science, 1888, XXXVI, 3rd Ser., pp. 359-379; also *Philos. Mag.*, 1889, XXVII, 5th Ser., p. 1; and *Invisible Solar and Lunar Spectra*, *Philos. Mag.*, 1888, XXVI., 5th Ser., pp. 505-520.

source. For medium intensities, for example, the curve is highest in the yellow-green and lowest in the blue. But as the intensity is decreased the curve levels, while its maximum height shifts to the green and its minimum to the red. In short, the photometric value of a color is not a constant but a variable function of its intensity. From the above consideration, it is obvious, then, (a) that the photometric method can not be used to estimate the relative intensities of the colors of the spectrum even for a single intensity of light-source unless for each point of the spectrum considered a factor be determined which will transform the photometric into the radiometric value; and (b) that it can not be used over a wide range of intensities of light-source unless this calibration be previously made for each degree of intensity used. Furthermore, the brightness factor is not inseparably bound up with the intensity factor. It can be isolated. When, for example, one uses a constant amount of colored light, spectral or pigment, and mixes with it a constant amount of white, black, or gray, one obtains stimuli which contain an equal amount of colored light but which have different brightnesses. Up to the present this method of isolating the brightness factor to test its influence has been employed only by Hess, and in his work, as we shall see, it has been used very inadequately.

The discussion whether the brightness of the stimulus affects color sensitivity was raised by Aubert. Aubert was unable to reach positive conclusions concerning its influence. He was led to a consideration of the question by the outcome of an investigation planned to determine the influence of visual angle upon the perception of color in central vision. His results showed that the liminal visual angle was different in case of the different colors, and further, that it differed when the colors were viewed upon white and black grounds. In the discussion of these results he writes: "Zum Theil beruhen diese Verschiedenheiten wohl auf einer verschieden starken Affection des Farbensinnes, zum grösseren Theil aber wohl auf Helligkeitsdifferenzen. Wir haben dabei drei Momente zu berücksichtigen, nämlich die Farbennüance, die Farbenintensität, und die Helligkeitsdifferenz oder den Contrast der Pigmente."<sup>58</sup> These *Momente* Aubert defines as

<sup>58</sup> Aubert, H. *Physiologie der Netzhaut*. Breslau, 1865, p. III.

follows: *Farbennüance* is the sensation given when a color is mixed with white, black or gray. A light blue, for example, contains more white and less blue light than a saturated blue.<sup>59</sup> *Farbenintensität* is defined as "the impression which is dependent on the intensity of colors: in case of spectral colors, on the amplitude of vibration; in case of pigments, on the intensity of the illumination." "Maxwell," he says, "calls this '*shade*': one color may be lighter or darker than another."<sup>60</sup> Whether or not Aubert also uses the term *intensity of color* as synonymous with *white-value* is open to question. There is evidence in his discussion, however, that he uses it synonymously with *brightness*. For example, when discussing *Farbenintensität*<sup>61</sup> he claims that the intensity factor cannot be made standard because it is impossible to determine which of two colors is the brighter, and because the photometric values of the spectral colors is unknown, the results of Melloni, Dove, and Helmholtz on this point differing widely. But whether or not *brightness* means for him also *white-value* depends upon what he thinks is measured by the photometric method. *Helligkeitsdifferenz* is the brightness relation between a color and its background.<sup>62</sup> The three factors, then, that Aubert believes we have to consider are (*a*) the amount of colorless light mixed with the color; (*b*) the intensity or brightness of the color; and (*c*) its contrast with the background. The first of these factors, he contends, influences color perception. He finds that the more colorless light is mixed with a pigment color, the the greater must be the illumination at which the color can be liminally sensed; in other words, when the pigment surface reflects a small amount of colored light, the intensity of its illumination must be proportionately greater to give liminal sensation.<sup>63</sup> The third of these factors he also considers very important. Colors have different limens and limits of visibility on white and on black grounds. Now the brightness of the

<sup>59</sup> Aubert, H. loc. cit.

<sup>60</sup> Aubert, H. op. cit., p. 108.

<sup>61</sup> Aubert, H. op. cit., p. 111.

<sup>62</sup> Aubert, H. op. cit., p. 112.

<sup>63</sup> Aubert, H., Untersuchungen über die Sinnesthätigkeit der Netzhaut. Pogg. Ann. d. Physik und Chemie, 1862, CXV, p. 111, 114.

color determines how great the difference is in the two cases. Blue, for example, is very dark. It is in great contrast to the white field and in much less contrast to the black. Its limit in each case is respectively  $15^{\circ}$  and  $36^{\circ}$ ,—a difference of  $21^{\circ}$ . Red is less dark. Its limits with the white field is  $16^{\circ}$ , with the black  $30^{\circ}$ ,—a difference of  $14^{\circ}$ . Green is a lighter color and more nearly of mean brightness between white and black. In this case the difference in its limits with white and black fields is only  $4^{\circ}$ . Aubert contends “dass die spezifische Farbenwahrnehmung an der Seitentheilen der Netzhaut um so eher in eine blossse Wahrnehmung von Hell und Dunkel übergeht, je stärker dieselbe mit der Umgebung contrastiert.”<sup>64</sup> This is stated again in the *Physiologie der Netzhaut*: “Contrast und Helligkeit der Farben sind von grossem Einfluss auf die qualitative Farbenempfindung, so wie auf die Grösse der Netzhautparthie, innerhalb welcher die Farben empfunden werden sein.”<sup>65</sup> In this regard, then, the difference in the brightness of the colors has an effect on color sensitivity only when the brightness of the surrounding field is made the same for all of the colors. But since it need not and should not be made the same for all of the colors in any investigation of sensitivity, unless the purpose of the investigation is to test the effect of surrounding field on the different colors, this case may be ruled out of consideration. The second *Moment*, the intensity or brightness of the colors, Aubert finds himself unable to isolate. He works with pigment stimuli and can not alter the intensity without at the same time changing the proportion of colored to colorless light reflected from his stimuli; or as he calls it, the nuance of the color. That is, in order to change the intensity of the color, he is compelled to change the amount of white, black, or gray mixed with it.<sup>66</sup> Further, he thinks it is often impossible to determine which of two colors is the brighter.<sup>67</sup> However, his experiments to determine the relative intensity of illumination necessary for the

<sup>64</sup> Aubert, H. Ueber die Grenzen der Farbenwahrnehmung auf die seitlichen Theilen der Netzhaut. A. f. O., 1857, III,<sub>2</sub>, p. 54.

<sup>65</sup> Aubert, H. Physiologie der Netzhaut. Breslau, 1865, p. 122.

<sup>66</sup> Aubert, H. op. cit., p. 153.

<sup>67</sup> Aubert, H. op. cit., p. 111.

liminal visibility of the different colors lead him to think that the brightness of color is not in itself a factor. In these experiments he finds that when the colors are arranged in order according to the intensity of illumination necessary for their perception, they are from least to greatest: orange, yellow, red, light blue, light green, blue, green. But when the illumination is decreased so that all the stimuli are seen as grays, they differ in brightness from light to dark in the sequence: yellow, light blue and green, blue, green, orange, red. Since the order differs in the two cases, he concludes that the differences in the perceptibility of the colors can not be due to their difference in brightness. He says: "Vergleicht man diese Ordnung der Farben nach ihrer Helligkeit mit ihrer Reihenfolge hinsichtlich ihrer Erkennbarkeit bei beschränktem Lichtzutritt, so sieht man, dass die Helligkeit der Pigmente nicht die Ursache seyn kann, dass diese oder jenes Farbenquadrat bei einer geringeren Lichtintensität farbig erscheint."<sup>68</sup> His final conclusion from this discussion of the three *Momente* is that the colors are not equally perceptible; but it can not be determined in how far this depends upon color tone, color intensity, or color nuance.<sup>69</sup>

In clearing the ground by the discussion of these three *Momente*, Aubert, then, finds himself utterly unable to answer the question: Does the brightness difference between two colors affect the sensitivity of the retina to these colors? because he can not isolate the factors involved for investigation. He, however, recognizes the possibility that brightness difference may affect color sensitivity and for this reason is inclined to think that his own may be only a rough determination. He writes: "To obtain a fine estimation, the influence of brightnesses must be eliminated; and pigments of equal intensity and nuance must be observed upon a background of the same brightness as the pigment. But we do not possess such pigments; and since the photometric value of prismatic colors is not known, an exact estimation of the liminal visual angle at which the colors can be sensed seems impracticable."<sup>70</sup> As will be shown later, Bull

<sup>68</sup> Aubert, H. Pogg. Ann. d. Physik und Chemie, 1862, CXV, p. 105.

<sup>69</sup> Aubert, H. Physiologie der Netzhaut. Breslau, 1865, p. 123.

<sup>70</sup> Aubert, H. op. cit., p. 112.

and Hegg take this statement as authority to equate colors in brightness for investigations of the peripheral limit of color sensitivity. It is obvious, however, that the statement contains no real authority for the equation of colors to be used even in investigations of color sensitivity in central vision, because Aubert confesses that he is unable to demonstrate whether or not the brightness of a color affects the retina's sensitivity to that color. He has merely expressed a belief that in fine determinations the equation should be made. Baird, as we have seen, also claims Aubert as authority for equation in brightness, but apparently he has not even this much basis upon which to rest his claim, for in Baird's long list of references to Aubert, none is found to the section containing the statement quoted above. This statement occurs in Aubert's discussion of the influence of visual angle on the perception of colors in direct vision (*Physiologie der Netzhaut*, pp. 108-115). Baird's references to the *Physiologie der Netzhaut* are pp. 89-105; 116-124. As has been shown on p. 13, Baird apparently gets his authority from a discussion in which Aubert is clearly concerned not with the white-value of color but with the total effect of changes in the general illumination. Furthermore, so far as the writer has been able to ascertain, Aubert nowhere else gives as much authority for equating in brightness as is contained in the statement selected by Bull and Hegg upon which to base their claim.

Aubert's opinion that the effect of the brightness of a color upon the retina's sensitivity to that color can not be determined was also held by Chodin who was unable to isolate brightness and intensity from each other.

Chodin writes:<sup>71</sup> "Es bleibt nur übrig die Farben bei gleicher Sättigung und bei mittlerer Lichtintensität zu vergleichen, und da sie unter dieser Bedingung von verschiedener Intensität sind (vielleicht sind die idealen Farben von gleicher Helligkeit, wie Hering sich vorstellt aber wir wissen dies nicht und können es nicht wissen) so liegt es sehr nahe anzunehmen, dass diese verschiedene Helligkeit eine constante Eigenschaft der Farbe selbst sei, welche, wenigstens in merklichen Grade, weder vermehrt

<sup>71</sup> Chodin, A. op. cit., p. 178.

noch vermindert werden kann ohne eine Veränderung des Charakters der Farben selbst herbeizuführen." Chodin goes on to point out one of the difficulties of attempting to obtain stimuli of equal brightness. In case of some of the colors, both spectral and pigment, a change of quality or tone takes place, when, in their state of greatest saturation, their brightness is altered. For example, spectral yellow when darkened gives a reddish-yellow or brown appearance, and the yellow of pigment paper, a decided olive-green appearance. Blue, when altered in brightness, appears reddish. These color changes have been recorded by Chodin, Brücke,<sup>72</sup> Hegg,<sup>73</sup> Rood,<sup>74</sup> and others. They are marked and are particularly troublesome in that the blue and yellow stimuli, which undergo the greatest changes in quality, are the stimuli that must be altered the most in order to be equated in brightness.

Ole Bull<sup>75</sup> was the first to mention the problem under discussion in its specific relation to the determination of peripheral color limits. He made no direct test of the influence of the brightness of the stimulus upon sensitivity, but quoted as authority for equating his own stimuli Aubert's statement that for fine determinations of color sensitivity brightness differences must be eliminated. Like Aubert, he was unable to show that the brightness of the stimulus constitutes a factor, but unlike Aubert he does not recognize the need for demonstrating this point. He thus equated his stimuli apparently without ever even having realized the need to investigate whether or not this equation should be made.

Hess<sup>76</sup> seems to have been the only investigator who has made any attempt whatever to determine whether a light and a dark color of equal intensity have the same or different limits of visibility in the peripheral retina. His test was simple and was applied only to one color. Using two stimuli, the one composed

<sup>72</sup> Brücke. Wiener Berichte, 1865, LI, p. 10.

<sup>73</sup> Hegg, E. loc. cit.

<sup>74</sup> Rood. On the Effects Produced by Mixing White with Colored Light. Phil. Mag., 1880, X., p. 209.

<sup>75</sup> Bull, O. op. cit., p. 93.

<sup>76</sup> Hess., C. op. cit., p. 42.

of  $180^\circ$  *Urgün* and  $180^\circ$  white, the other of  $180^\circ$  *Urgrün* and  $180^\circ$  black, he compared their limits in the horizontal temporal meridian. In each case the stimulus was exposed on a background whose brightness was the same as that of the darker stimulus. Hess found the limit of the dark green to be  $25^\circ$ , of the light green  $13^\circ$ . The value of this brief test lies in that it shows a recognition of the need to investigate whether or not the brightness of the stimulus affects the limits of sensitivity, before an attempt is made to equate in brightness. Hess's method of determining the influence of the factor is, however, open to criticism in two regards. In the first place he made no determination of the amount of brightness difference existing between the colors used. He arbitrarily altered the brightness of green  $180^\circ$  in both directions, that is, toward both black and white. He gives no reason for this choice of  $180^\circ$  of variation of the brightness component. He does not tell us, for example, whether it represents as much brightness difference as there exists between his stimuli, or whether it represents more, or less. If it had represented less, the test would not have possessed due rigor. If, on the other hand, it had represented more, the test would not have been fair for two reasons. (a) Brightness mixed with color inhibits color sensation. This inhibitive action is greater the nearer the brightness mixed with the color approaches white, and is less the nearer the brightness approaches black.<sup>77</sup> It is obvious, then, that the difference between the amounts of the inhibition of the color by the white and that by the black would increase as the amount of the brightness component increases. If, then, in the above test, Hess had mixed with *Urgrün* in turn amounts of white and black that represented a brightness variation greater than the brightness difference that exists between the standard colors, he would have had a greater difference between the amounts of inhibition in the

<sup>77</sup> As will be shown in the experimental section of this paper, this may be stated as a general law of the action of brightness on all colors for all parts of the retina with the exception of a region within  $5^\circ$  of the limits of sensitivity to red and yellow, as determined with stimuli of normal saturation and intensity. Within this narrow zone the saturation of red and yellow is reduced more, apparently, by black than by white.

color mixed with white and the color mixed with black, and consequently a greater difference between the intensities of the sensations aroused by them, than he would have had, if a brightness change equal to that existing between the standard colors had been used. His results, then, can not be taken as evidence that the proper amount of brightness change necessary to render the standard colors equal in brightness would change the limits of color sensitivity. Much less can they be taken to show that the amount of brightness difference actually existing between the standard colors acting in conjunction with the greater amount of light coming from the standard colors than from his weakened stimuli would affect the limits of sensitivity.

(b) In Hess's experiment, the color stimulus containing  $180^\circ$  *Urgrün* and  $180^\circ$  white, and that containing  $180^\circ$  *Urgrün* and  $180^\circ$  black both possessed less physical intensity than would stimuli in which less brightness variation had been made. That the degree of intensity of the stimulus is a potent factor in the determination of whether or not the difference in brightness between a light and a dark color of equal physical intensity affects the peripheral limit of sensitivity to that color will be shown in the experimental part of this paper (pp. 104 ff.). The results of this work, however, may be anticipated briefly at this point. As has been stated, the action of white when mixed with color inhibits the apparent saturation of the color more than black does. The question under consideration is whether the apparent saturation of a dark color is sufficiently greater than that of a light color of equal physical intensity to widen the peripheral limit of sensitivity to the dark color. Now a determination of the limen of sensitivity to color at a number of points from the fovea to the limits of sensitivity to color shows that the sensitivity to a color decreases slowly from the fovea to a point about  $5^\circ$  within the limit of sensitivity to that color as determined with a stimulus of full saturation,—the point varying slightly for the different colors in its distance from the color limit. From this point on toward the limit, the sensitivity falls off very rapidly. A large increase in the limen is present from degree to degree in this region, often as great as  $90^\circ$  per degree of retina traversed. It

is obvious, then, that if the intensity of the colored stimulus is sufficient to cause its limit to fall within this zone of rapid decrease in sensitivity, the difference in saturation between a light color and a dark color of equal physical intensity would not be sufficient to cause a widening of the limits. On the other hand, if a smaller intensity of color were used so that the limit of the color occurred within the retinal region where color sensitivity alters but slightly from point to point, the greater apparent saturation of the dark color would be sufficient to allow the color to be visible at a point where the light color of less apparent saturation though of equal physical intensity is not sensed. Now the limits for the different colors when mixed with white at the point where the rapid decrease in sensitivity to each begins, is as follows:  $120^\circ$  of yellow;  $130^\circ$  of green;  $135^\circ$  of red; and  $145^\circ$  of blue. These values, then, represent the amount of color that would have to be had in the stimulus to which white had been added to make the limits of sensitivity to them fall just at the boundary between the zones of gradual and abrupt decrease in sensitivity. And since black inhibits color less than white, even less color would be required in the stimuli to which black had been added to make the limits fall at this point. For any amount of color greater than these values, the limits would fall within the zone of rapid decrease. One could, then, add white to his stimuli by amounts varying from  $0^\circ$  to  $215^\circ$ - $240^\circ$  for the different colors, and black in still greater amounts, and still work within the zone of abrupt decrease. And as long as one works within the region of abrupt decrease, the limits for the colors lightened and darkened by the above amounts will coincide. It need hardly be said that  $215^\circ$ - $240^\circ$  multiplied by two to express the variation in both directions, represents a much greater brightness difference than exists between the standard colors. On the other hand, for any amount less than the above values, the limits would fall in the zone of gradual decrease. Now Hess's stimuli contained less than this amount of color because his *Urgrün* contained only  $244^\circ$  of green and  $116^\circ$  of blue.  $180^\circ$  of this *Urgrün* would, then, contain but  $122^\circ$  of green and  $58^\circ$  of blue. The intensity of this stimulus was, therefore, so small that ac-

according to our results the limits for both component colors would fall in the zone of gradual decrease of sensitivity. In this zone the greater saturation of the color when mixed with black would cause it to be sensed farther out than the color when mixed with an equal amount of white; that is, the limits for the darkened color would fall further from the fovea than the limits for the light color, as Hess found them to do. In this regard, too, then, it may be said that if in Hess's test, a brightness change was made greater than the brightness difference between the standard colors, and consequently a stimulus was used having smaller intensity than it would have had, if the proper brightness change had been made, the test would have been unfair because the degree of intensity of the stimulus is a factor determining whether or not there is a different limit for light and dark colors of equal physical intensities.

While it is sufficient for our point to show in what respects Hess is open to criticism for arbitrarily selecting  $180^\circ$  of brightness variation without making any determination of how much need be made, a word may be added to show that  $180^\circ$  of brightness variation is much more than was needed. For, the table in which Hess<sup>78</sup> states the values of the stimuli determined by different observers to be equal in cancelling-action and brightness, shows that in no case was a variation of more than  $133^\circ$  made in one direction, that is, toward either white or black. It is often difficult to see just how much variation was made, because in the majority of cases both black and white were added, hence the net variation was less than the value of either. For example,  $105^\circ$  of yellow,  $60^\circ$  of white, and  $195^\circ$  of black were found by one observer to be equal in cancelling action and brightness to  $85^\circ$  of blue,  $84^\circ$  of white, and  $191^\circ$  of black; for another observer,  $200^\circ$  of red,  $48^\circ$  of blue,  $66^\circ$  of black, and  $46^\circ$  of white were equal in cancelling action and brightness to  $188^\circ$  of green,  $39^\circ$  of blue, and  $133^\circ$  of black. But in no case is the variation in one direction more than  $133^\circ$ , or less than  $91^\circ$ . We may conclude, then, that by his choice of  $180^\circ$  of brightness variation by means of which to compare the limits of a light and

<sup>78</sup> Hess, *op. cit.*, pp. 45-46.

a dark color of equal physical intensity, he has, roughly speaking, made his brightness variation one and one-half times greater and his intensity only four-fifths of what it should have been, judging from the greatest variation he had to make for his brightness equation. Either one of these items: the overestimation of brightness change or the underestimation of intensity is sufficient both to render his test unfair and, moreover, to account for a very considerable difference in the limits for color mixed with white and for the same color mixed with black.

The second point of criticism of Hess's test is in regard to his method of controlling the brightness of the surrounding field and of the preëxposure. He did not use the proper conditions of brightness of screen and of preëxposure card. He used a screen and a preëxposure which were of the same brightness as the dark stimulus, both in the tests with the dark and with the light color. There was, then, a considerable amount of white added to the light stimulus by contrast from the dark background and by after-image from the dark preëxposure over and above the white that was added by objective mixing. Thus while he aimed to add equal amounts of white and black to his pairs of stimuli, he really added much more white than black. Now, we have found that white subjectively aroused apparently has as much effect on the color excitation as white objectively aroused. His test was thus again rendered unfair by his lack of proper conditions with regard to the brightness of the surrounding field and of the preëxposure. The surrounding field and the preëxposure should have been made in each case equal in brightness to the stimulus.

Hess does not appear to have realized the importance either of the brightness of the surrounding field or of preëxposure as factors influencing color limits. He generally equalized the brightness relation between stimulus and background because he seemed to think that accuracy of judgment was fostered thereby,—that one can more readily tell when the color has disappeared from a given stimulus when there is no brightness difference between the stimulus and the background to confuse the judgment.<sup>79</sup> The preëxposure he made of the same quality

<sup>79</sup> See Hess, *op.cit.*, p. 25.

and brightness as the background. No reason was given for this procedure.<sup>80</sup> Thus in the above test, he overlooked the diminution in the intensity of the sensation aroused by the lighter stimulus due to the contrast from the surrounding field and to the after-image from the preëxposure,—a diminution which is also in itself sufficient to account for quite a large difference in the limits for a light and for a dark stimulus of the slight degree of intensity used by Hess. Hess's results, then, assuredly can not be considered as showing that the brightness difference between the normal colors affects their limits of sensitivity.

Hegg,<sup>81</sup> like Bull, made no test to determine whether the brightness of the stimulus constituted a factor in the determination of color limits. He gives no reason for the attempt he makes to equate in brightness, other than the fact that Aubert and Bull had mentioned the necessity for this procedure.

Baird was the next to state that the stimuli used to investigate the peripheral color sense must be of equal brightness. His reasons for making this statement have already been discussed in part (pp. 12-19) in order to show how the prevailing confusion with regard to terms has led to misinterpretation. To connect this preceding discussion with what is to follow, a few words to résumé will probably be of service here. It will be remembered that like Bull and Hegg, Baird made no attempt to determine whether or not the brightness or white-value of a color exerts an influence on the limits of sensitivity to that color. Unlike Bull and Hegg, however, he claims to be able to derive authority for equating stimuli in white-value from the work of many investigators,—primarily from that of Aubert,<sup>82</sup> Landolt,<sup>83</sup> and Abney,<sup>84</sup> but also from Raehlmann,<sup>85</sup> Chodin,<sup>86</sup> Klug,<sup>87</sup> Bull,<sup>88</sup>

<sup>80</sup> See Hess, *c. op. cit.*, p. 44.

<sup>81</sup> Hegg, *E. op. cit.*, p. 146.

<sup>82</sup> Baird, *J. op. cit.*, p. 12.

<sup>83</sup> *ibid.*, p. 16.

<sup>84</sup> *ibid.*, p. 31.

<sup>85</sup> *ibid.*, p. 17.

<sup>86</sup> *ibid.*, p. 20.

<sup>87</sup> *ibid.*, p. 20.

<sup>88</sup> *ibid.*, p. 22.

Hess,<sup>89</sup> and Hegg.<sup>90</sup> With regard to these sources of authority, it has been shown (a) that Abney and Landolt do not even claim that brightness difference affects the sensitivity of the retina to color, and that Aubert does not in the references given by Baird; (b) that Bull and Hegg equated their stimuli in brightness merely because Aubert had expressed the belief that such procedure is necessary in making fine determinations of color sensitivity, but since Aubert was unable to demonstrate this necessity, their reason for making the equation has no value;<sup>91</sup> and (c) that Hess's test, upon the results of which he bases his conclusion with regard to the need to equate, was both incomplete and wrongly devised. We have yet to show, then, that no authority can be derived by Baird from the work of Raehlmann, Klug, and Chodin. A claim to authority was derived from the work of Raehlmann and Klug by misinterpretations similar to those made in the cases of Landolt and Abney. Raehlmann and Klug both worked with spectral light and sought to find the effect of decreasing the intensity of the colored light upon the limits of sensitivity. Raehlmann<sup>92</sup> decreased the intensity of his stimuli as follows: Light reflected from a heliostat was passed through a prism and the spectrum from this source was thrown upon a screen, which may be called screen<sub>1</sub>, the distance of which from the light-source was kept constant. This screen contained an opening for the transmission of the colored light. The amount of light transmitted could be regulated by the size of this opening, and the quality could be regulated by shifting the position of the opening along the spectrum. The colored light fell upon a second screen, screen<sub>2</sub>, so arranged that its distance from screen<sub>1</sub> could be varied. In two ways, then, could diminution of

<sup>89</sup> Baird, J. op. cit., p. 27.

<sup>90</sup> *ibid.*, p. 29.

<sup>91</sup> As has been shown, pp. 40-44, Baird, as well as Bull and Hegg, might have had some justification in citing Aubert's authority on the question of brightness equation, from the latter's statement that all brightness differences must be eliminated from stimuli used to make fine determinations of color sensitivity. But this statement of Aubert's is not included in the references to Aubert from which Baird drew his authority.

<sup>92</sup> Raehlmann, E. Ueber Verhältnisse der Farbenempfindung bei indirectem und directem Sehen. A. f. O., 1874, XX., p. 18.

intensity be produced: (a) by decreasing the size of the opening in screen<sub>1</sub>, and (b) by increasing the distance between screen<sub>1</sub> and screen<sub>2</sub>. Both methods were used by Raehlmann for producing what he terms *die Abnahme der Lichtstärke*. A change in brightness may have been incidentally produced, but this aspect of the stimulus was of no concern to him. He found that a decrease in intensity decreased the zone in which a color was sensed in its characteristic tone; he most assuredly does not claim, however, as Baird says he does that "the color limits were found to vary with changing brightness of stimulus" (see Baird, p. 17) in the sense in which Baird uses brightness, namely, as white-value. Klug<sup>93</sup> used a method somewhat similar to Abney's. He weakened a beam of light by interposing respectively one, two, and three thicknesses of ground glass, and found that the color limits were narrowed in each successive case. Thus he also made no attempt to isolate the effect of the brightness of the stimulus, and his work can not be cited as having any bearing on that problem. In Chodin's work, as we have already seen, the advisability of equating in brightness was discussed and decided against because of lack of evidence for the need of equating and because of the changes in color tone produced by changing the brightness of the colors. In giving Chodin as one of his authorities for equating, Baird refers to the passage in Chodin's article quoted in the original in this paper, pp. 44-45.

Baird writes: "Chodin remarks in his introduction: 'It is self-evident that in comparing the retinal sensitivity to different colors, the color stimuli employed must be of equal brightness and of equal saturation.' But this very essential condition was not fulfilled in his own experiments" (see Baird, p. 20). Baird has here again made a misinterpretation. The rather free translation of Chodin's statement: "Es bleibt nur übrig die Farben bei gleicher Sättigung und bei mittlerer Lichtintensität zu vergleichen"<sup>94</sup> and the failure to read carefully the discussion following it, are responsible, we presume, for the misinterpretation.

It is obvious from the foregoing résumé that the factor, bright-

<sup>93</sup> Klug, F. Ueber Farbenemfindung bei indirectem Sehen. A. f. O., 1875, XXI., pp. 274-278.

<sup>94</sup> Chodin, A. op. cit., p. 178.

ness of the stimulus, has been very inadequately treated in the literature. The specific question has never been answered, in fact has never really been investigated: Does the amount of brightness difference existing between the colors influence their limits of sensitivity in the peripheral retina? Aubert<sup>95</sup> and Chodin<sup>96</sup> and others have shown that the sensation limen of color when mixed with white is higher than the limen when mixed with black. This fact may be explained as due to the superior inhibitive power of white. But within what limits this greater inhibitive action of white is sufficient to cause the peripheral limit of a color mixed with white to be narrower than that of an equally intense color mixed with black has not been determined. And certainly it has never been shown that the brightness difference that exists between the standard colors at full saturation exerts an inhibitive action sufficiently strong to cause a change in the peripheral limits. It has never been claimed, for example, that a light color in its state of maximal saturation is more inhibited for sensation than a dark color in its state of maximal saturation by the brightness component inherent in each; in other words, that a saturated yellow is more inhibited by its brightness component than is a saturated blue by its brightness component. In fact, in strange contradiction to this, it has often been held that the colors which have the stronger white component are the more intense. Yellow, for example, has been frequently called a more intense color than blue just because of its proximity to white.

We must conclude, then, that the assumption that color limits must be investigated with stimuli of equal brightness is probably based upon the belief that stimuli differing in brightness differ also in intensity. This belief has doubtless arisen from the fact that as stimuli are ordinarily varied, a change of brightness is accompanied by a change of intensity, and conversely a change of intensity is accompanied by a change of brightness. But brightness and intensity are not inseparable variants. Conclusions should not be drawn, therefore, until the influence of bright-

<sup>95</sup> Aubert, H. *Physiologische Optik*, p. 532.

<sup>96</sup> Chodin, A. *op. cit.*, p. 183.

change has been investigated in separation from intensity change. Since this investigation has not been made, we are forced to consider that the influence of the brightness of the stimulus upon the limits of color sensitivity is at present an open question, despite the verdict to the contrary by Bull, Hegg, Hess, and Baird. We have, therefore, included it in our own work as one of the points to be investigated. The results of this investigation are reported in the experimental section of this paper.

### 3. *Brightness of the Field Surrounding the Stimulus.*

The recognition of the influence exerted by the field surrounding the stimulus upon the limits of the color zones, has led to the substitution of the campimeter for the perimeter in investigations of the color sensitivity of the peripheral retina. The campimeter provides a means of readily changing the brightness of the field which surrounds the stimulus, so that the effects of these changes may be studied both upon the limens and limits of color and upon the quality changes that appear as the stimulus is carried from the fovea to the periphery.

The influence of the field surrounding the stimulus is twofold. In the first place, it directly modifies the stimulus by contrast induction, provided there is brightness opposition. This effect was observed and to some extent investigated by Aubert and Woinow before the campimeter came into use. In the second place, the campimeter screen, when of sufficient size, stimulates the entire retina uniformly and guarantees an equal brightness-adaptation of every portion. It was the recognition by Krükow<sup>97</sup> that former methods had allowed the retina to become unequally fatigued to chance objects in the surrounding room, that led directly to the first use of the campimetrical method of working. Krükow did not, however, study the effect of different backgrounds. He used a uniform gray field to stimulate the surrounding retina, and mounted stimuli on cards of equal quality. In subsequent investigations, a black background was used almost exclusively. So far as induction is concerned,

<sup>97</sup> Krükow, loc. cit.

this screen gives conditions with the light-adapted retina somewhat similar to those existing in dark-room work; that is, in each case the stimuli are lightened by contrast from the surrounding dark field.

Woinow and Aubert worked only with small areas of background and thus secured the given brightness stimulation over but a small zone surrounding the part of the retina stimulated to color. Woinow<sup>98</sup> placed a disc made of black and white sectors behind the stimulus to be investigated. He found that, when the sectors were so adjusted that when rotated they formed a dark gray, the color limits were the same as when the sectors were arranged to give a light gray sensation. From these results, he concludes that the color zones are not influenced by the brightness of the field surrounding the stimulus. Aubert<sup>99</sup> fastened colored paper stimuli on white and on black cards. He found that the black card gave relatively wider limits for red; and that the white card gave relatively wider limits for yellow, green, and blue, except for very small stimuli.

The first campimeter described was apparently what is now called the Hering color-mixer.<sup>100</sup> Hess<sup>101</sup> was the first to employ it for an investigation of peripheral color sensitivity. He and later Tschermak<sup>102</sup> tested by means of it the influence of the brightness of the surrounding field upon the color limits. With this apparatus pigment stimuli are observed through an opening in a large gray screen, placed in the horizontal, which can be turned toward or away from the source of light, and in this way a surrounding field can be obtained that is lighter, darker, or equal in brightness to the stimulus at its point of disappearance as color. Hess and Tschermak both found that the limits of sensitivity to

<sup>98</sup> Woinow, M. loc. cit.

<sup>99</sup> Aubert, H. *Physiologische Optik*, pp. 541-543.

<sup>100</sup> Titchener in *Experimental Psychology, Instructor's Manual, Qualitative*, 1901, p. 20. ascribes the description of this apparatus to Hering, giving as reference A.f.O., 1889, XXV., p. 63. The writer is unable to find any mention of this apparatus in this or any other of Hering's articles. It is, however, described in some detail on p. 25 of the paper by Hess which just precedes and accompanies the Hering article to which Titchener refers.

<sup>101</sup> Hess, C. loc. cit.

<sup>102</sup> Tschermak, A. op. cit., p. 561.

color were widest when the surrounding field was equal in brightness to the stimulus. If the stimulus appeared lighter or darker than the surrounding field, the limits were narrowed proportionately to the loss of saturation of the stimulus color due to the action upon it of the brightness quality induced by the background.

Fernald<sup>103</sup> used a vertical campimeter. She summarizes her results with white and black screens as follows: "All the colors except the reds are perceived at a greater angle of eccentricity with the dark than with the light backgrounds."

The only quantitative estimates of the effect of different backgrounds reported by these experimenters is given in terms of the effect upon the color limits. In no case has the amount of white or black induced by a given screen been determined, nor has the effect of the induction upon color sensitivity ever been tested in any part of the retina by the most direct means available, namely, the determination of the limen or threshold of sensation. Neither has any attempt been made to isolate the influence of the background from the influence of the brightness of whatever stimulates the retina immediately before the exposure of the stimulus. This factor, which we shall discuss under the name of *preëxposure*, is effective through the intensive brightness after-image that is set up on the retina and is superimposed upon the colored stimulus when it is exposed. Its importance has never been recognized by previous investigators, nor has its effect ever been studied in isolation from the effect of the brightness of the background. In short, in surveying the literature, one can scarcely help but feel that the study of the influence of the surrounding field has been neither analytic nor systematic.

#### 4. *The General Illumination.*

The effect of the general illumination of the retina on color sensitivity has been recognized since the time of Purkinje and Aubert. It has been studied in some detail by a number of experimenters, among whom may be mentioned Kramer and Wolffberg. Both have shown that the sensation aroused by the colored stimulus is weakened by a reduction of the general

<sup>103</sup> Fernald, G. M. The Effect of Achromatic Conditions on the Color Phenomena of Peripheral Vision. Psychol. Rev. Monog., 1909, X, No. 42.

illumination, but neither, it may be mentioned, has given a method of keeping the general illumination constant. Kramer's<sup>104</sup> purpose was to determine the sensitivity of the eye under different intensities of daylight and artificial illumination. His method was as follows. Stimuli, 4 mm. square, of blue, yellow, red, and green paper on a black background were used. The distance at which the stimulus had to be placed from the observer to be just recognized as colored, was tested by sunlight and when the sky was obscured by clouds and for three intensities of each of the following sources of artificial illumination: candle light, gas, petroleum, sodium, potassium, strontium, and calcium lights. His results may be summarized as follows: (1) Red is seen at the greatest distance in all lights except calcium, in which case green is seen when farther away than red. The other colors are recognized in the order green, yellow, blue. (2) All the colors are recognized at a greater distance when seen by sunlight than when illuminated by artificial light or the dull light from a clouded sky. (3) As the intensity of the artificial illumination is decreased, the colors must be placed nearer the eye to be recognized. Kramer's method of working, however, may be criticized because he ignored the white contrast which the black background induced across the stimuli. The induction across the stimuli whose sizes were only 4 mm. square must have been considerable. It was, moreover, of different amounts in each case; because brightness contrast is greatest when there is maximal brightness opposition. The modification of the light colors, as a result of contrast induction, must, therefore, have been greater than that of the dark colors.

Wolffberg's<sup>105</sup> interest was in the influence of gradual alterations of the general illumination on the light and the color sensitivity of the central and of the peripheral retina. His room was illuminated by daylight entering through a window. Fifteen different degrees of illumination were produced by fastening from one to fifteen thicknesses of tissue-paper over the win-

<sup>104</sup> Kramer, J. Untersuchungen über die Abhängigkeit der Farbenempfindung von der Art und dem Grade der Beleuchtung. Inaug. Diss., Marburg, 1882.

<sup>105</sup> Wolffberg. Ueber die Prüfung des Lichtsinnes. A. f. O., 1887, XXXI., pp. 1-78.

dow. The illumination obtained when the window was uncovered was called 15/15; when covered with one thickness of tissue-paper, 14/15, etc. His method of determining the effect of variations of illumination upon the central retina was as follows: Pigment stimuli were placed at a standard distance of 5 meters from the observer, and the size of stimulus necessary to render it just visible in its true color was determined. In the peripheral retina, he investigated to what extent the limits of white and of colored stimuli were altered by reducing the illumination. In all his experiments, the stimuli were fastened on a black background. Wolffberg's results for the central retina are shown in the following table. The stimuli were circular in shape and of diameters given in columns 2, 3, 4, 5, and 6.

Illumination	Size of Red Stimulus	Size of Blue	Size of Green	Size of Yellow	Size of White
15/15	.5 mm.	3 mm.	3 mm.	1.5 mm.	.2 mm.
14/15	1.5	5	4	2	.5
13/15	2	6	6	4	1.
12/15	2.5	12	12	4.5	2
11/15	3	20	20	5	2.5
5/15	10	50	50	10	6

These results show that in the central retina a decrease of illumination has a greater effect upon the sensation of color than upon the sensation of white. Wolffberg next tested the effect of a gradual decrease of illumination upon the limits of sensitivity to white and to the colors. He found that the extent of the visual field was not narrowed for white when the illumination was decreased to 1/15. The color limits, however, narrowed gradually when the illumination was decreased from 15/15 to 3/15. The narrowing was in no case more than 15°. The relative extents of the fields remained unaltered, that is, the order of size was in every case blue, red, and green.

Although special investigations have been conducted by Kramer, Wolffberg and others to show the effect of changes in the general illumination upon color sensitivity, in general little if any precautions have been taken by earlier experimenters to prevent such changes when investigating color sensitivity. Either the experimenter has not considered the influence of the

general illumination, or he has been satisfied to take the rough precaution to work only on bright days at stated hours. Ole Bull,<sup>106</sup> for example, commented at length on the factor of general illumination, but suggested no method for its standardization. He writes: "The amount and nature of the general illumination are of more significance in perimetrical observations than one is accustomed to consider. It must always be noted whether the sky is clear or cloudy, whether it rains or snows. The extreme limits of the visual field for mixed light undergo such wide fluctuations that it is of little value to establish an average limit on the basis of a number of measurements. Changing illumination, conditioned by the time of day and of year during which the work is carried on, as well as the locality in which it is undertaken, produce variations in the same stimulus large enough to cause differences of from  $10^{\circ}$  to  $20^{\circ}$  [in the limit of sensitivity]. Especially in the nasal parts of the retina does the illumination influence the color limits, while their position remains more constant in the temporal retina." Fernald,<sup>107</sup> however, did make some attempt to obtain a standard illumination. She arranged white curtains at the windows of her optics-room which could be lowered on bright days and drawn on dark days. This rather crude method was used also by Thompson and Gordon.<sup>108</sup> It is scarcely necessary to point out that the method lacks the first essential of standardization, namely, a means of measuring.

It is surprising that Wolffberg as the logical corollary of his work, did not draw attention to the importance of standardizing the illumination of the visual field in all work on the color sensitivity of the retina, and show how it could be accomplished by a modification of his method of working. He already had at hand one of the essentials for standardizing, namely, a method of changing the illumination of his room. The other essential, a method of measurement by means of which an illumination could be identified with a previous illumination chosen as standard,

<sup>106</sup> Ole Bull. *Perimetrie*. Bonn, 1895, p. 8.

<sup>107</sup> Fernald, G. M. *Psychol. Rev.*, 1905, XII, p. 392.

<sup>108</sup> Thompson and Gordon. *A Study of After-images on the Peripheral Retina*. *Psychol. Rev.*, 1907, XIV, p. 122.

might have been derived from his results. For example, it would seem to have been a simple matter for him to have chosen as standard the particular illumination at which the red stimulus of 2.5 mm. diameter, the blue and green of 12 mm. each, the yellow of 4.5 mm., and the white of 2 mm. were just recognizable at a distance of 5 m. Stimuli of those sizes, it will be seen from the tables, were just recognizable at this distance at the illumination called 12/15, when 15/15 represents the illumination "bei günstige Tagesbeleuchtung." Using this condition as an index of the standard illumination, he could at any time have adjusted the illumination of the room by adding to or subtracting from the layers of tissue-paper covering the window, until the stimuli of these sizes were again just recognizable at the given distance. The accuracy and sensitivity of this method could have been tested by comparing the results of a series of determinations. An accurate and highly sensitive method sustaining some similarity in principle to the method suggested here, will be described by the writer in the experimental part of this paper.

The influence of changes in the intensity of the general illumination upon visual acuity has received some attention from physiologists and oculists. Although their work has no direct bearing on the influence of change of illumination upon color sensitivity, it may be of interest to note briefly their methods of dealing with these changes.

Schweigger<sup>109</sup> in 1876, using the Snellen series of optotypes and the formula  $V = \frac{N}{n}$  in which  $n$  represents the distance of the test-object from the eye of the observer, and  $N$  the number of the smallest of optotype series that can be recognized at that distance, found that on a clear day his visual acuity equalled 20/15, on a cloudy day it equalled 30/15. To correct for the errors in visual acuity introduced by changes in the illumination he first found the number of the series of the smallest optotypes that he himself could read at a given distance, then he determined this value for the patient at the same distance. Using his own results  $V = \frac{N}{n}$  as standard, he determined the ratio of the patient's results  $V = \frac{N^1}{n}$  to his own. This ratio  $\frac{N^1}{N}$ , he considers the expression of what the patient's visual acuity would be at standard illumination.

Cohn's and von Hoffman's interests lay mainly in testing the eyes of schoolchildren and in determining what was the lowest intensity of illumination of the schoolroom suitable for work. Cohn<sup>110</sup> in 1867 and 1883

<sup>109</sup> Schweigger, E. *Sehproben*. Berlin, 1876, Preface, pp. III-IV.

<sup>110</sup> Cohn, H. *Untersuchungen der Augen von 10060 Schulkindern*. Leipzig, 1876, p. 101; *Hygiene of the Eye in Schools*, translated by Turnbull, 1883, p. 131.

claims that as there is no photometer available for the measurement of the intensity of daylight, the eye must be its own photometer. Later in 1892<sup>111</sup> he states that L. Weber has made a daylight photometer, but as this apparatus is difficult of access, he would recommend apparently that the changes in visual acuity experienced by the eye with changes of illumination be used as a means of identifying a given degree of illumination. He endorses von Hoffman's<sup>112</sup> method of accomplishing this. According to this method, Type No. 30 of the Snellen optotypes is placed in the schoolroom 15 feet from the eyes of a child whose visual acuity is 15/15. If the child recognizes the letters of the test, the room is sufficiently well-lighted. Work in the room is to be suspended as soon as the child can no longer recognize the letters of the test. This provided a practical method, not for measuring the illumination of a room, but for detecting when a room has insufficient light for purposes of schoolwork.

Nicati<sup>113</sup> tested the influence of change of the intensity of artificial illumination upon visual acuity. His work was purely quantitative. He proposes a unit of measure by means of which to study this effect. This unit he calls a *photo*. A *photo* is the smallest intensity of light which when placed 1 meter from a test-object printed in black on a white card gives to normal monocular vision a normal acuity. The method of measuring the intensity of an illumination in *photos* is as follows. A source of light is brought towards the test-object until the observer has normal acuity. The intensity of the source then equals as many *photos* as the square of the distance of the light-source from the test-object, measured in meters. Nicati finds that there is an absolute logarithmic relation between visual acuity and intensity of illumination. As visual acuity is decreased in arithmetical series, intensity of illumination decreases in geometrical series. His table showing this relation is as follows:

Visual Acuity	1	.9	.8	.7	.6	.5	.4
Distance of source	1M		2M		4M		8M
Intensity in <i>photos</i> .	1	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$	$\frac{1}{16}$	$\frac{1}{32}$	$\frac{1}{64}$

Since this relation exists, either the intensity of illumination can be considered a measure of visual acuity, or visual acuity can be considered a measure of the intensity of illumination. That is, the scale of visual acuity is a photometric scale and can be used as such. To measure the illumination of a room in *photos*, then, the visual acuity should be determined in different portions of the room, and the average of the *photos* corresponding to these values in the acuity scale be taken as the measure of the illumination of the room in *photos*. The method as formulated is apparently serviceable as a means of estimating the illumination of a room chiefly, if not entirely, when it is below what is needed for normal acuity of vision.

<sup>111</sup> Cohn, H. Lehrbuch der Hygiene des Auges. Wien und Leipzig, 1892, pp. 34-35.

<sup>112</sup> von Hoffman, H. Augenuntersuchungen in vier Wiesbadener Schulen. Klin. Monatsbl. f. Augenheilk., 1873, pp. 289-290.

<sup>113</sup> Nicati, W. Physiologie Oculaire humaine et comparée normale et pathologique. 1909, p. 163ff.

## B. METHODS OF STANDARDIZING THESE FACTORS.

1. *Size of the Stimulus.*

No special method of standardizing the size of the stimulus is required. Each experimenter who has recognized it as a factor has chosen what he considered the most favorable area to work with, and has used that area in all of his comparative determinations. No limitations as to area have been prescribed other than that it must not be too large nor too small.

2. *Intensity of the Stimulus.*

As a general introduction to our discussion of the methods that have been used to standardize the influence of intensity, we wish to call attention once more to the fact that in no one of these investigations have the stimuli employed been equated with regard to the energy of the light given to the eye, nor have they been standardized in terms of any fixed unit of intensity that can be compared; and that until this is done, we have no proper means of determining the comparative limits of sensitivity to the different colors; nor of determining and expressing the comparative limen or j. n. d. of sensitivity. For example, attempts to determine the relative sensitivity of the retina to the four principal colors have been made among others by Aubert, Chodin, Raehlmann, Butz, Lamansky, and Dobrowsky: also by Bull, Hess, Hegg, and Baird. As stated above, however, these experimenters did not equate their stimuli with regard to the energy of light given to the eye for the investigations of the limits of sensitivity nor estimate sensitivity in terms of a common unit for the work on limens and j. n. d.'s of sensitivity. The last four, however, did attempt to equate in intensity, but the equation was made in terms of a subjective measure arbitrarily selected, namely, the proportion in which the pairs of antagonistic colors must be combined to produce gray, or, in terms of the Hering theory, to cancel each other.

At this point, two general criticisms may be passed on this method of equating. (a) The stimuli should not be equated in terms of any subjective measure if one is to test the comparative sensitivity of the retina to the different colors. This begs

the question at the outset. If, for example, a direct judgment of the intensity of the sensations aroused by two stimuli either at the limen or higher in the intensity scale be taken as the criterion for determining their equality, the method begins by making the stimuli of such physical intensity that they are sensed equally. In no fashion could the comparative sensitivity of the retina to the colors in question be determined by such stimuli. Suppose, for example, the limits of sensitivity of the peripheral retina were to be investigated, and for this, the stimuli which had been made subjectively equal for the central retina were used, the results obtained would not at all express the comparative limits for the colors in question. If these limits should be found to coincide, the conclusion could not be drawn either that the sensitivity of the retina to these colors extended only to this point, or that there was equal sensitivity at this point to the colors used. At most no more could be said than that approximately the same ratio of sensitivity to the two colors obtained in this region that was present at the point in the central retina for which they were equated; but this ratio may not be a 1:1 ratio. In fact, the investigator who gets his limits to coincide with stimuli so equated finds himself in the somewhat ludicrous position of having made his conditions such that the limits could not help but coincide, regardless of whether they actually ought to do so or not. He is not working with real limits, but with limits arbitrarily established, and the coincidence he finds is not a fact, at least not so far as he is able to determine by his method of working, but an artifact. To illustrate, the retina might be much more sensitive to red than to green; but if the red stimulus were reduced in physical intensity until the sensation it aroused was equal in intensity to the sensation aroused by the green, it is obvious that the comparative sensitivity to these colors could not be directly tested at any point. The limits of the red zone so determined might, for example, coincide with the limits of the green zone, although the extent of the red zone would have been much wider had stimuli of equal physical intensity been used. So much may be said for this as a type of subjective measure of equality, and what is said in criticism of its use for

investigating the comparative sensitivity of the retina has a general application to all subjective measures.

(b) The stimuli, especially should not be equated in terms of the cancelling power of antagonistic colors.<sup>114</sup> This method is anomalous. One scarcely knows what it does accomplish. On the one hand, stimuli so equated are in no way equated in physical intensity; and on the other hand, it would be the merest assumption to say that they have equal power to arouse sensation. To demonstrate this, let us compare certain colors with regard to their comparative power to arouse sensation and to cancel each other. There are three ways by which we may judge the power of a color to arouse sensation:— (a) the value of its limen when mixed with a gray of equal brightness; (b) the value of its j. n. d. at different points in the intensity scale; and (c) the direct or introspective judgment of its intensity. Estimated in all of these ways, spectral red, as prepared in pigment colors by Maxwell after Helmholtz, has greater power to arouse sensation than the green of the series. And yet on the color-mixer, it requires  $240^\circ$  of this red to cancel or obliterate all trace of  $120^\circ$  of the green. And if sufficient blue be added to the mixture to give gray, the proportions of red and green are  $165^\circ$  and  $115^\circ$  respectively. Thus, whether the result of the combination is yellow or gray, the red stimulus, although it has the greater power to arouse sensation, is required to be in considerable excess of the green. We regret that we can not make a similar comparison for the blue and yellow of the Maxwell series, because we were unable to procure them in season for this work. But using the blue and the yellow of the Hering pigment papers, we find that this blue has a lower limen in a gray of its own brightness than the yellow has in a gray of its brightness, and that introspectively it is judged much more saturated than the yellow. And still at the general illumination we have chosen as our standard,  $200^\circ$  of blue are required to cancel  $160^\circ$  of yellow.<sup>115</sup>

<sup>114</sup> There are problems in the optics of color in which subjective equations of intensity may be desired. For a description of what the writer considers a proper method of making the subjective equation, see this paper p. 83, footnote.

<sup>115</sup> If the illumination is decreased, we find that a different proportion

In a later paper, we shall show that if the complementary colors can be assumed to cancel each other, because a certain amount of the one when combined with a certain amount of the other, kills it for sensation, then, by the same token, the non-complementary colors may be assumed to exercise a degree of this cancelling action. For a definite and considerable amount of each must be mixed with any other before it is sensed, the amount required varying over a wide range when one of them is combined with each of the others in turn. We may draw upon the non-complementary colors, then, for a further demonstration of our thesis that the cancelling power of colors upon each other is no measure of their power to arouse sensation. A notable instance of this is the combination of red and yellow to give orange. Working with the Hering pigments, we find that the standard orange of the series, judged as sensation, seems to be equally red and yellow. The standard red of the series, however, which is chosen to form the orange, has greater power to arouse sensation than the yellow by all of the tests mentioned above. Still  $295^\circ$  of red are required to be combined with  $65^\circ$  of the yellow to give the orange which as we have said is equally red and yellow as sensation. Orange furnishes us with only one instance of the non-equivalence of cancelling power to sensation-arousing power that may be found among the combinations of non-complementary colors.<sup>116</sup>

We must, then, conclude that even if one were to err so profoundly as to choose a subjective measure for equating the intensity of his stimuli for an investigation of the comparative is required to produce cancellation. This difference depends upon the fact that colors do not lose their saturation with equal rapidity with decrease of illumination. The dependence of cancelling proportions upon the general illumination may be pointed out as a minor source of error in the use that has been made of this method by investigators who have conducted their experiments in daylight, for they did not work at an invariable or standard illumination.

<sup>116</sup> In the above demonstration that cancelling power can not be taken as the equivalent of sensation-arousing power, we have assumed that we have not left our work open to criticism in the use of pigment instead of spectral colors, or even in passing from one series of pigment colors to another, because in each case, our test of the cancelling power and of the sensation-arousing power was made with the same stimuli.

sensitivity of the retina to the different colors, this measure should not be selected in preference to the power to arouse sensation as determined either by limen tests, by direct introspective judgments of intensities, nor by the j. n. d. method described on p. 83 footnote. One can only surmise the following reasons for the selection. (a) The limen and the just noticeable difference tests, either of which is a proper method of estimating the power of a stimulus to arouse sensation, were probably not taken into consideration at all. The alternative test, the introspective judgment of equal intensities, is difficult to make. And the method given on p. 83 footnote, which is preferable to any method known to the writer for subjectively equating stimuli of all degrees of intensity, has never been suggested even in principle prior to the publication of this paper. It may have been thought that equality based on cancelling power could be substituted for the equality as determined by the other methods.

The reason for selection of equation in terms of cancelling power is not stated by any one of the four men who has made this selection. Considering the statement of each in turn, we find that Bull who wished to obtain color stimuli of equal saturation and brightness, merely states that he does this by establishing pairs of colors such that equal amounts of each cancel and give a gray that conforms to the standard gray he has chosen (A. f. O., 1881, XXVII., p. 95). Hess, aiming to obtain a red light such that its red *Valenz*, or, as he says, its capacity to arouse red sensation, is equal to the green *Valenz* of a green light, writes: "Als Nächstliegende erscheint es nun, einem roth- und einem grün-wirkenden Pigmente den gleichen Roth und Grünwerthe dann zuzuschreiben, wenn dieselben zu gleichen Theilen eine farblose Mischung geben" (A. f. O., 1889, XXXV., p. 39). Hegg wishing to equalize the color-values of his stimuli, claims that this is possible for only the two members of each pair of antagonistic colors. Red can be made equal in color-value to green, he says, or yellow to blue, but green and blue can not be equalized. He continues: "Wir betrachten ein *Roth* und ein *Grün* als chromatisch äquivalent, wenn sie auf der Rotationsscheibe zu gleichen Theilen gemischt sich gegenseitig total aufheben, so dass eine Mischung entsteht, welche weder ins rothliche noch grünliche sticht." (A. f. O., 1892, XXXVIII., p. 149). Baird offers no reason whatever for selecting the method of cancelling power by means of which to equate color-values. He apparently takes for granted that this method is the only one, and does no more than describe how the method was applied to his particular stimuli.

The search for a reason for this selection may, however, be pushed back a little further. We find that in 1880 before any of the work mentioned above was published, Hering (Lotos. Jahrbuch für Naturwissenschaft,

1880, I, pp. 76-107) had made statements from which we can conclude that in his opinion two antagonistic colors of equal sensation-arousing power cancel. Hering's statements leading to this conclusion are as follows: (a) 'Die Vermögen der Lichtstrahlen, die weisse Empfindung zu fördern, will ich die weisse Valenz der Lichtstrahlen nenne.' (p. 79.) (While this definition is not specifically repeated for colored light, still it is obvious from the text that it applies to colored as well as to white light).<sup>117</sup>

<sup>117</sup> The following quotations are appended to show the use of *Valenz* by Hering and Hess:

Hering (Zur Erklärung der Farbenblindheit aus der Theorie der Gegenfarben. Lotos—Jahrbuch für Naturwissenschaft, 1880, I., p. 76-107) writes (p. 79): "Die Vermögen der Lichtstrahlen, die weisse Empfindung zu fördern, will ich die weisse Valenz der Lichtstrahlen nennen.

"Die Grösse dieser Valenz ist offenbar von zwei Factoren abhängig: ersten von der objectiven Intensität oder lebendigen Kraft, mit welchen die Strahlen verschiedener Wellenlänge bis zur empfindlichen Netzhautschicht gelangen, und zweitens von dem, was wir die specifische weisse Erregbarkeit des Sehorgans gegenüber den Strahlen verschiedenen Wellenlänge nennen, d.i. das Vermögen dieses Organs, unter dem Einflusse jener Strahlen die Weissempfindung deutlicher werden lassen.

"Ausser der weissen Valenz, welche allen Lichtstrahlen gemeinsam ist, kommen nun den einzelnen Strahlenarten verschiedene farbige Valenzen zu. Allen Strahlen vom aussersten Roth oder vom Anfange des Spectrums bis zu jenem im Tone reinen Grün, welches eine Grundfarbe ist und welches wir das Urgrün nennen wollen, haben eine gelbe, allen Strahlen vom Urgrün bis zum violetten Ende des Spectrums eine blaue Valenz."

Hess (Ueber den Farbensinn bei indirectem Sehen, A. f. O., 1889, XXXV., pp. 1-60.) writes (p. 30): "Unter weisser Valenz eines farbigen homogenen oder zusammengesetzten Lichtes versteht Hering den Helligkeitswerth desselben für eine Netzhautstelle, welche das farbige Licht wegen mangelhaften Farbensinnes oder aus anderen Gründen farblos sieht" (p. 39): "Um über das gegenseitige Verhältniss der Abnahme der Empfindungsvermögens für Roth und Grün, resp. Blau und Gelb überhaupt Untersuchungen anstellen zu können, ist es zunächst erforderlich, für beide Arten des Empfindungsvermögens ein gemeinsames Maass zu finden. Verschiedene grüne Lichter besitzen die Fähigkeit, grüne Empfindung zu erzeugen, in sehr verschiedenem Maasse, sie sind, um es kurz zu bezeichnen, sehr verschieden grünwirkend. Das mehr oder minder grosse Vermögen eines Lichtes, grün zu wirken, bezeichnen wir mit Hering als die grüne Valenz oder den Grünwerth des bezüglichen Lichtes". (p. 40): "Bestimmen und messen lässt sich derselbe nur in Bezug auf ein als Normalgrün gewähltes Pigment, welches unter genau denselben Beleuchtungsverhältnissen wie das zu untersuchende gesehen wird. Ganz analoges gilt von dem Roth-, Gelb-, und Blauwerthe eines Pigmentes.

"Für die vorliegende Frage handelt es sich aber nicht bloss darum die Grünwerthe oder die Rothwerthe verschiedenen Pigmente je unter sich zu vergleichen, sondern den Grünwerthe eines grünwirkenden mit dem Rothwerthe eines rothwirkenden Pigmentes.

"Als das Nächstliegende erscheint es nun, einem roth- und einem grünwirken-

(b) "Zwei homogenen Lichter, nun, von welchen das eine ebenso gelb (oder roth) wirkt, und das andere blau (oder grün) so dass beide Valenzen sich aufheben, nenne ich gegenfarbig äquivalent" (pp. 83-84). In the first of these statements he directly calls the capacity of a color to arouse sensation its *Valenz*. And from the second it may readily be derived that when the yellow-sense, for example, is affected as strongly by yellow light as the blue-sense is affected by blue light, complete cancellation will ensue,—that is, equality in cancelling power may be considered as the equivalent of equality in capacity to arouse sensation. In making this deduction we have of course assumed that *wirkt* refers to sensation-arousing action and not to cancelling action. We have no doubt that this assumption is correct, still it may be worth while to bring forward direct evidence in support of this point from a statement made by Hess while working under Hering's direction. Hess writes: "Das mehr oder minder grosse Vermögen eines Lichtes grün zu wirken, bezeichnen wir mit Hering als die grüne Valenz oder den Grünwerth des bezüglichen Lichtes" (op. cit., p. 39). Here *Vermögen grün zu wirken* is made the equivalent of *Valenz* and *Valenz* by definition is the capacity of a color to arouse sensation. Hence we have little hesitation in assuming that in the case in question *wirkt* also refers to the sensation-arousing action of the colored light and not to its cancelling action, and in concluding, therefore, that Hering believed that antagonistic colors of equal power to arouse sensation would also have equal power to cancel each other. Since this is true, it is probable that the followers of Hering (Hess and Hegg) assumed the equivalence of power to arouse sensation and power to cancel and equated their stimuli accordingly. That Hess was actuated by some such reason is shown by a statement made by him in his discussion of this point. He writes: Die von Herrn Professor Hering angegebene, oben geschriebene Untersuchungsweise gestattet mit grosse Genauigkeit den zu vergleichenden Pigmenten gleich grosse farbige und gleich grosse weisse Valenz zu geben, sie ermöglicht es, für die Werthigkeit der Farben einen genauen numerischen Ausdruck zu winnen und in die Rechnung einzuführen" (op. cit., p. 58). Hegg also seems to refer back to Hering, for he uses the Hering terminology in discussing the equation of his stimuli.

Or (b) since cancellation is the corollary to the assumption of an assimilation-dissimilation mechanism, it may have been considered for some reason, not readily understood by the writer, that an equation based upon it is the proper one to make.

Having said this much about the impropriety of selecting a subjective measure for the intensity equation of stimuli, let us

den Pigmente den gleichen Roth-und Grünwerth dann zuzuschreiben, wenn dieselben zu gleichen Theilen, z.B. auf dem Kreisel gemischt eine farblose Mischung geben, im Falle sie dazu aber in einem anderen Verhältnisse gemischt werden müssen, anzunehmen, dass sich der Rothwerthe des einen zum Grünwerthe des anderen umgekehrt verhält wie die Grösse der beiden zur Herstellung einer farblosen Mischung nöthigen Sektoren."

pass to a résumé of the attempts that have been made to apply this measure by Bull, Hess, Hegg, and Baird. Hegg selected four stimuli that suffered no alteration of color tone in passing from the center to the periphery.<sup>118</sup> These were a bluish-red, a bluish-green, a blue, and a yellow. They were equated in pairs, the bluish-red to the bluish-green, and the blue to the yellow, as follows. It was determined in what proportions the members of each pair had to be combined to produce gray, and from these proportions, values of the sectors of the stimulus disc were calculated for each color. The procedures of Bull and Hess were essentially similar..

Baird, employing the light transmitted by gelatines, prepared blue, yellow, red, and green stimuli as follows. A lantern containing an incandescent lamp of 16 candle-power was used as source of light. The stimulus light was emitted from the lantern through a circular aperture, 15 mm. in diameter. Gelatines were placed over the aperture in combinations which gave the four stable colors, and their spectral values were obtained. A disc in which two windows of equal size had been cut, was rotated on a motor in front of the lantern. The combination of gelatines to give the red stimulus was fastened across one of the windows, while the green combination was used to cover the other window. As the windows were of equal size, the rotation of the disc gave a mixture which contained equal proportions of both stimuli. The gelatine combinations were changed by adding, subtracting, or substituting until the mixture showed no trace of color. Similar equations were obtained for the blue and the yellow stimuli.

It will be seen from the work of these men that even if their methods had been based upon a proper principle of equating, they would not be adequate for all that is involved in the problem

<sup>118</sup> Only one meridian was used for determining this invariability of color tone. It is obvious that a conclusion should not be drawn from such a scant investigation of the sensitivity of the retina. For example, working with the red, green, blue, and yellow of the Hering standard papers, the writer has found that with a careful standardization of factors, an investigation in any considerable number of meridians shows that stability of tone is possessed by the blue alone.

of determining the comparative sensitivity of the retina to the different colors. For not only is the comparative sensitivity to the complementary colors desired, but to the non-complementary colors as well. The method offers no possibility, for example, of equating red and green to blue and yellow. One can only conjecture how much of our present conception of the comparative extent of the different zones of color sensitivity is an artifact due to the use of stimuli that have not been equated with reference to the energy of the light-waves they give to the eye. In addition, then, to the objection that the methods that have been used thus far to equate the color stimuli in intensity are found to be essentially wrong in principle, the further criticism may be offered that they are not adequate in scope. An energy equation of the light-waves by means of some radiometric device, for example, the thermopile, the bolometer, the selenium cell, or what not, alone seems adequate to the requirements set by the problem of determining the comparative sensitivity of the retina to the different colors, or the comparative limits of the zones of sensitivity.

Energy equations in terms of radiometric units have been made by Langley and Pfund, but up to this time no investigation of color sensitivity has been made with colors equalized in energy. Langley<sup>119</sup> invented the bolometer and determined by means of it the relative distribution of energy in the normal spectrum. In order to equalize the energy of the different colors, he states that one may vary the width of the collimator-slit until equal radiometric readings are obtained. In his own experiments on visual acuity, he does not, however, proceed in this way. Tables of logarithms were illuminated in a dark-room by monochromatic light representing known amounts of energy. The greatest distance at which the figures could be read was determined for each of the colors, and corrections were applied for inequalities in the energy of the different lights. The corrections were made in terms of the distribution obtaining in the following table.

Pfund used the first method suggested by Langley. In an

<sup>119</sup> Langley. *Energy and Vision*. *Amer. Journ of Science*, 1888, XXXVI., 3rd Ser., pp. 359-379.

investigation of the changes in the resistance of selenium to lights of varying wave-length, he employed differently colored

Wave-length	$\mu$ .35	$\mu$ .38	$\mu$ .45	$\mu$ .50	$\mu$ .55	$\mu$ .60	$\mu$ .65	$\mu$ .70	$\mu$ .75	$\mu$ .768
Heat	1.8	5.3	11.9	17.3	20.7	21.9	22.2	21.4	20.7	20.2

beams of equal intensity. The intensity equations were made as follows. Using first a Rubens thermopile<sup>120</sup> and later a radio-micrometer,<sup>121</sup> Pfund determined which wave-length gave the least galvanometer deflection. He then reduced the more intense beams by interposing a smoked wedge of the proper thickness until every portion of the spectrum produced the same deflection. In this way he obtained colored lights of known and constant energy.

Psychological investigators have been slow to recognize the importance of standardization of intensity in radiometric terms of the colors which are to be used for the investigation of sensitivity. The only equations of intensity have been made in subjective terms, a procedure which if done by a proper method may be legitimate for work on certain points relative to existing color theories, but which is not adequate (see this paper, pp. 64-65) to meet the requirements of the problems which deal with the comparative sensitivity of the retina to the different colors.

Note.—Since the completion of this paper, the report of Watson and Yerkes concerning methods of studying vision in animals has been published (*Behavior Monographs*, 1911, I, pp. 1-89). For the measurement of the intensity of the stimulus they find two methods available, photometry and radiometry. They write: "The method of photometry in all its forms is dependent upon the visual capacity, training, and the special skill of the observer who attempts to use it. For this reason, and others only less important, it is usually desirable to supplement photometric measurements of photic stimuli by measurements of their value in terms of energy. Hence the pertinence of physical measurements. Determination of the value of photic stimuli in terms of heat units by radiometric procedure has proved feasible. Radiometry yields a measurement which is relatively independent of the visual peculiarities of the observer, and it therefore supplements in an invaluable manner the results of photometry" (p. 11).

The authors in question then decide in favor of radiometric measurement

<sup>120</sup> Pfund, A. A Study of the Selenium Cell. *Philos. Mag.*, 1904, VII, Ser. 6, p. 26.

<sup>121</sup> Pfund, A. The Electrical and Optical Properties of Metallic Selenium. *Phys. Rev.*, 1909, XXVIII, p. 326.

and control of the stimuli to be used in determining the animal's color sense. Their reasons for this decision are not, however, those stated in the above criticism of subjective methods of equation either by cancelling power or by sensation-arousing power, namely, that these methods are essentially wrong in principle for tests for the comparative sensitivity of the eye to different colors. That they do not consider them wrong in principle for work of this kind is shown in fact by their recommendation of the Hegg colored papers. The colors of the Hegg papers are equated in intensity in terms of the cancelling power of the complementary colors, the worst of the subjective methods discussed. They write: "These [the Hegg papers] are mixtures of oils on paper yielding the hues red, yellow, green, and blue. These hues are claimed to be equal in intensity and saturation for the human eye. The set is useful as a means of ascertaining, in a preliminary survey, whether an animal readily discriminates two hues which for us are of nearly the same intensity and saturation" (p. 32). It is obvious also that they do not consider the photometric method of equating intensities (also a subjective method) wrong in principle. The method is not recommended merely because it depends upon the visual capacity, training, and special skill of the observer. But the fact that they endorse this method to supplement the radiometric procedure or rather, as quoted above, the radiometric to supplement the photometric shows that they do not realize the absolute diversity of the photometric and of the radiometric curve. Their conclusion, then, in favor of the method of radiometry for measuring the intensity of the stimulus is based upon very different arguments from those which have governed the similar decision reached in the above discussion. They do not seem to entertain any criticism of the subjective method of equating, either the method which measures cancelling power, or the method of photometry, nor do they recommend that either be discarded. Their choice of energy measurement is due largely to the fact that they wish a method which is as free as possible from subjective errors.

### 3. *Brightness of the Stimulus*

The same investigators who sought to obtain stimuli of equal intensity, attempted also to equate these stimuli in brightness. This may be done in two ways: the white-values of the colors as they appear in direct vision may be equated, or the white-values as they appear in indirect vision may be equalized. The first method was used by Bull who made direct comparison judgments of the relative brightness of the colors, facilitating his comparisons by the use of intermediate color-tones. For example, a blue was changed in brightness until it appeared as light as a given blue-green. Green was then made equal to the blue-green; yellow-green to the green; yellow, to the yellow-green, etc. Hess, Hegg, and Baird employed the second

method. The stimuli were carried to a point in the field of the peripheral retina at which they appeared colorless and their brightness values were altered until the gray sensations obtained from all the stimuli were equal.

Hegg, who used pigment colors, observed the stimulus through an opening in a gray screen, whose brightness could be altered by turning it toward or away from the source of light. He adjusted the screen so that its brightness was the same as that of the gray sensation aroused by the green stimulus in the peripheral retina. Retaining this setting of the screen, he replaced the green by the red stimulus, the intensity of which he had previously equated to the intensity of the green by the method described and criticised in the preceding section. The red stimulus, which was composed of  $216^\circ$  of red,  $55^\circ$  of blue,  $89^\circ$  of white, when observed in the extreme periphery, was seen as a gray that was lighter than the screen. To make the stimulus and the screen of equal brightness,  $5^\circ$  of the white sector had to be replaced by black. A complication arose when blue and yellow were equated to this brightness, resulting from the changes in color-tone which took place. Hegg found that when he added white to lighten the blue stimulus, a sensation of reddish-blue was aroused. (Chodin, it will be remembered (see p. 45), saw in this fact an argument against the possibility of equating the brightness of colors for investigations of this kind.) To cancel this effect, he added green. The addition of black to yellow, which was necessary in order to equate the brightness of yellow to green, resulted in a greenish-yellow sensation. To this he added red in a sufficient amount to cancel the greenish appearance of the fusion.<sup>122</sup>

<sup>122</sup> In connection with a study (done in cooperation with Dr. C. E. Ferree) to determine the physiological level at which the fusion of colored with colorless light sensation takes place, the writer attempted to add sufficient red to cancel the green in a mixture of yellow and black. A curious paradox was observed. Starting with  $55^\circ$  of yellow, and  $305^\circ$  of black, and keeping these proportions relatively constant while red was being added to the mixture, it was reported by a number of observers that, after the addition of about  $10^\circ$  of red, it was seen in the mixture with the green. As more was added, the green and red continued together in varying proportions, until, with about  $45^\circ$  of red in the mixture, it dominated the fusion, which was seen as a dark brownish-orange. Our ob-

Hegg does not give the proportions of the final white-green-blue *Urblau* and the black-red-yellow *Urgelb* which, he claims, were equal in brightness to the *Urroth* and *Urgrün*.

Baird also used the method of indirect vision comparison. The two stimuli to be equated were placed one above the other at a point at which both appeared colorless in the periphery of the retina. The brightness of blue was chosen as standard, and the red, green, and yellow stimuli were darkened to equal it by rotating an episcotister in front of each of them in turn. The sectors of the episcotister were adjusted so that each stimulus was darkened as much as necessary to cause the colorless sensation aroused by it in the periphery to be the same as was aroused by the blue stimulus. Baird does not say that his work was complicated by changes in color-tone. His method would at first glance seem to be more simple than that of Hegg. When, however, we remember that the equation of brightness and apparent intensity had to be carried on hand in hand, we see some of the difficulties he must have encountered. His problem, was to bring the complementary colors to such intensity, that  $180^\circ$  of one cancelled  $180^\circ$  of the other; and at the same time, to maintain them all of the brightness of the blue. But it is apparent that by his method of equating in brightness, an alteration in the amount of colored light coming to the eye is produced every time a change in brightness is made. And as the brightness of the several stimuli had to be changed by unequal amounts to bring them all to the brightness of blue, the amount of colored light coming to the eye was also changed by unequal amounts. This much of the procedure is sufficient to show the difficulty that confronts the experimenter. To equate either for brightness or cancelling power, disturbs the equation established for the other; that is, when the stimuli are brought to equal brightness, their cancelling power will no longer be equal, and *vice versa*. It is obvious that

servation has been verified too many times and by too many observers for us to question its validity. It stands, then, in direct contradiction to Hegg's claim that a change in color-tone produced by altering the white-value of a color can be remedied by adding the complementary color to the stimulus. The difficulty then, seems insurmountable, and stands as one of the objections to the attempt to equate colors in brightness.

the goal desired, if it can be attained at all, must be reached by a series of approximations; and that in the end the experimenter will have very much altered stimuli. Since to equate for both at once, involves making much more radical changes in the stimuli than to equate for one alone, it is plain that in doing both, we but add to the objections we have already made when each is done alone.

It is to be deplored that Baird does not tell us just how he worked in this most difficult part of his technique. The defect is serious, for as the report of his method stands, one can neither pass judgment on its adequacy, nor be sufficiently guided by it, should one attempt to repeat the work. Of the technique that is described, however, the following criticism may be offered. In the equation of the brightness of two stimuli, Baird carried them to an angle of excentricity, at which both appeared colorless. Now, it is seen from his tables, that the limit of blue is some  $15^\circ$  wider than that of green. He has, then, either to show that the brightness of green is the same at its limit as it is  $15^\circ$  peripheral-wards, or to equate the brightness of the colors at their individual limits by some means, such as the flicker method.

Since Bull equated his stimuli by the direct vision method, and Hess, Hegg, and Baird by the indirect vision method, a word may be said in concluding this topic with regard to which is the proper method. Obviously, the decision rests upon whether or not the colors have different relative white-values at center and periphery. That they do has been reported among others by Tschermak,<sup>123</sup> and we have been able to confirm this statement. The equation should, therefore, be made in the peripheral retina. As we shall show in the experimental section of our paper, however, no equation should have been made by any of these men for the work they were doing, because unless the stimuli used are extremely weak in saturation, to equate in brightness for the investigation of the limits of sensitivity not only is unnecessary, but results in positive harm. If, however, in other work in the peripheral retina the need for equating should arise, the writer would urge not only that the equation should be made in the

<sup>123</sup> Tschermak, A. op. cit., pp. 564-575.

peripheral retina, but that it should be obtained at the point at which the investigation is to be made.

*c. Summary.*

With regard to the attempts that have been made to standardize, the results of our historical survey are found to be largely destructive in character. They show, however, that a decided need for standardizing has been recognized. This in itself was a first step in the right direction. The following factors have been discussed: (a) *The size of the stimulus.* This factor has been the most adequately treated by previous investigators. Its influence as a factor has been shown, and with it the need of careful measurement of the actual size of the stimulus and of its apparent size as determined by its distance from the eye of the observer. There is still need, however, for further work. While it has been generally held that an increase in the area of the stimulus functions in some degree as the equivalent of an increase in intensity, and thus influences the limits and limens of color sensitivity, no quantitative estimate has been made of the degree of this equivalence. Exact knowledge of this point is not only of general interest in psychological optics, but it is needed in turn in certain problems of standardizing. For example, it is often required that the size of the stimulus be varied and its intensity for sensation be kept constant. This can be done only when the ratio of equivalence is known. As stated on p. 6, this ratio is now being worked out in this laboratory. The results will be reported later.

(b) *The intensity of the stimulus.* The influence of the intensity of the stimulus upon color limens and color limits has been pointed out, but no adequate standard of measure has been employed. In dealing with the comparative sensitivity of the retina to the different colors, estimated in terms of the limits, it is obvious that equal amounts of light should be used. Estimated in terms of the limens, the amounts used should be determined in terms of units that can be compared. The problem of the measurement of these amounts of light is wholly physical, hence the standard of the physicist should be adopted. The determination should be in terms of energy as measured by the bolometer,

the thermopile, or other radiometric device. Only in this way so far as we know, can the retina's sensitivity to the different colored lights be obtained in terms of units that can be compared.

(c) *The brightness of the stimulus.* Brightness and intensity have been much confused in the literature of the subject. The effect obtained by varying both factors has often been attributed to change in brightness alone. The effect of change in brightness has never been investigated in isolation. This factor, then, occupies the novel position of having been standardized for work on the limits of color sensitivity before the need for such control has been shown.

(d) *The preëxposure.* Only in a very general way has the effect of the brightness of the preëxposure been recognized, and the precise reason for its influence has been very little understood. No quantitative estimate of the effect has been made, and no attempt at standardization has been undertaken which has shown any comprehensive knowledge of how the factor works.

(e) *The field surrounding the stimulus.* Considerable attention has been given to this factor. A small amount of qualitative work has been done, and some attempts have been made to secure control of the factor. More detailed knowledge, however, is needed of its influence, quantitative and qualitative, over a wider range of the retina. Especially should its relation to general illumination be studied. Until this relation is understood and some means is taken to render the general illumination constant, no effective estimation of the influence of the brightness of the surrounding field can be obtained, nor can it be eliminated as a factor from the color observation.

(f) *The general illumination.* The influence of the illumination of the visual field on color sensitivity has been recognized and rough attempts have been made to determine the amount of this influence. The different ways in which changes in general illumination affect color sensitivity have not, however, been determined, and the relative importance of each has not been estimated. Very little attempt at standardization has been made because the first essential of standardization, namely, a sensitive means of measurement, has not been had.

## III. EXPERIMENTAL.

### A. PURPOSE OF INVESTIGATION.

The purpose of this investigation includes the following points. (1) The color observation will be analyzed for the brightness factors that influence its results. (2) A systematic study will be made of these factors with special reference to the determination of their effect upon the color sensitivity of the retina and upon the limits of sensitivity to different colors. (3) It will be ascertained whether the effect of these factors can not be explained in terms of the action of brightness upon color in the peripheral retina and of the rapidity with which the sensitivity to color decreases from the fovea outwards. (4) Methods will be devised to standardize these factors in so far as our results show the need of standardization. No attempt will be made at this point to study the factors that pertain to the source of light with the following exception. Brightness will be isolated from intensity and the effect on the limits of sensitivity of changes in the brightness of the stimulus, made without altering the amount of colored light coming to the eye, will be determined in order to find out whether or not colors should be equated in brightness when the limits of sensitivity are investigated. Moreover, since our problem is concerned only with the brightness factors that influence the action of the colored stimulus upon the retina, the writer will not feel obliged to concern herself with the standardization of her stimulus with regard to either quality or intensity any further than is needed to show the effect of the brightness factors upon the retina's response to these stimuli. All the standardization that is needed will be accomplished by using the same stimulus for all observations the results of which are to be compared; that is, no comparisons will be made except of the effect of the different brightness factors upon the same stimulus. For obtaining results so purely comparative the standardization afforded by pigment papers should be adequate, provided a standard illumination can be obtained so that the amount of

colored light reflected from the pigments will be constant from test to test. Since we were able to secure a highly sensitive means of duplicating our illumination from observation to observation, the standardization of the stimulus afforded by the Hering pigment papers has been considered adequate. More especially has this degree of standardization been considered adequate because the results are to be used primarily merely as a guide in the formulation of a method of working. Having secured a method of working, however, that will permit of a close duplication of results from observation to observation with the pigment papers, the writer will attempt to adapt the method to work in which the colors of the spectrum are used. In order to do this, the following requirements will have to be met. (1) A spectroscopic will have to be devised by means of which the retina can be stimulated at any degree of eccentricity in any meridian that is desired, for example, a spectroscopic that can be used in conjunction with the rotary campimeter<sup>1</sup> in all its adjustments. Such a spectroscopic having all the freedom of movements of its parts needed for use with the rotary campimeter has been devised in this laboratory and is now under construction. (2) In order that the stimulus-opening in our campimeter be filled with light sufficiently homogeneous for our purpose, a prism of high dispersive power will have to be procured for use in our spectroscopic. A compound prism of the Cassie type<sup>2</sup> seems adequate for this requirement. Such a prism constructed to our special order is now being made for us in Germany. (3) In order that the light may undergo high dispersion and still be sufficiently intense for work in a room lighted to the degree that some phases of our problem demand, a source of light of high intrinsic brilliancy is needed. Constancy in candle-power should also be had. A high voltage Nernst filament seasoned for 100 hours or more and operated on a steady circuit will give, the writer believes, the intensity and constancy required.

Having completed our work of standardizing the factors extraneous to the source of light, an attempt will next be made

<sup>1</sup>For a description of the rotary campimeter see this paper p. 87 ff.

<sup>2</sup>Cassie. *Philos. Mag.*, 1902, III. Ser 6., p. 449.

to secure a better control of the source. Standardization up to the present can be considered successful only with regard to the quality of light. No adequate work has been done on the standardization of the quantity of light for work on color sensitivity. As stated earlier in the paper, the writer believes that this can be done only by means of energy determinations. She expects to do her radiometric work by means of a surface thermopile (Coblentz model)<sup>3</sup> and a DuBois-Rubens *Panzer galvanometer*, unless future results show that some other combination of radiometer and galvanometer is more satisfactory.

Finally from the work of standardization it is our hope to return to the investigation of the problems which we were in the beginning forced to abandon because the work could not be satisfactorily done by the methods now in use in the optics of color. A brief statement of the plan of our future work has already been given in an article published in conjunction with Dr. Ferree in the *American Journal of Psychology*.<sup>4</sup> In order that the scope of this work be known at this point, and that the importance of the present investigation be understood in relation to this work, the statement is appended here.

"About a year ago<sup>5</sup> the writers undertook to determine the retina's sensitivity, relative and absolute, to colored light in terms of units that can be compared. Since several years will be required to complete this work, they have thought it best to publish a preliminary note showing briefly the purpose and scope of the investigation. The following points will serve to indicate what is being attempted in this study.

"(1) All measurements of sensitivity will be made in radiometric terms. This will give an expression of the sensitivity of the retina in units which are directly comparable with one another. At present we have no direct estimate of the comparative sensitivity of the retina to the different colors further than is ex-

<sup>3</sup> Coblentz, W. W. Instruments and Methods Used in Radiometry. Reprint No. 188, Bulletin of the Bureau of Standards, 1911, IX., pp. 22-23.

<sup>4</sup> Ferree, C. E. and Rand, G. A note on the Determination of the Retina's Sensitivity to Colored Light in Terms of Radiometric Units. *Amer. Journ. of Psychol.*, 1912, XXIII, pp. 328-332.

<sup>5</sup> The first public statement of our intention to use radiometric units in the investigation of the retina's sensitivity to color was made to the committee in charge of the Sarah Berliner Research Fellowship, February 1, 1911.

pressed, for example, by the relative width of the collimator-slit that has to be used to arouse color sensation when a light-source of a given candle-power is used. This kind of comparison is obviously unfair because such different amounts of energy are represented from point to point in the spectrum that a given width of slit would admit many times the amount of energy at one part of the spectrum that it would at another. In short, no adequate estimation and expression of the retina's sensitivity to color, comparative or absolute, can be made by means of the methods now in common use.<sup>6</sup>

"(2) Comparisons of results on many other points with such disparate stimuli seem equally inadequate: the relative time required for the different color sensations to attain their

<sup>6</sup>Two criticisms have been received from private sources which it may be well to take account of here. In one the possibility of a point of view is implied, in the other a point of view is stated. The point of view, the possibility of which is implied in the first criticism, is that it is not proper to estimate the sensitivity of the retina in terms of physical units, because it is generally conceded by modern investigators of color vision that the retinal processes which transform the physical energy of the color stimulus into nervous energy is essentially chemical in its nature; and one can not assume that a certain amount of physical energy arouses an equal amount of chemical energy in the retina, nor that equal amounts of physical energy arouse equal amounts of chemical energy. In answer to this, the writers would point out that these chemical substances are a part of the retina and their respective inertiae constitute one set of factors that determine the sensitivity of the retina to the different colored lights. It is not necessary to assume, therefore, that a given amount of physical energy arouses an equal amount of chemical energy, etc., in order to make our determinations of the comparative sensitivity of the retina to the different colors in terms of physical units. That would be necessary only if we were trying to separate out the nerve filaments, and to measure or compare their sensitivity to the different colors in terms of physical units. But even in chemical theories when speaking of the comparative sensitivity of the retina to the different colors, we do not mean the comparative sensitivity of the nerve filaments alone. We include the reaction of the chemical substances as well. Our contention, then, is that if the determination of the comparative sensitivity of the retina to the different colors is a proper problem, the determination should be made in terms of quantities that can be compared. This can be done either (a) by using lights equalized in energy and determining by means of a sectorized disc the relative amounts of these lights that are required to arouse sensation; or (b) by using lights representing different amounts of energy and measuring directly in terms of radiometric units the amounts required to arouse sensation. We scarcely need point out that in speaking of the comparative sensitivity of the retina to the different colors we are not raising a new problem, but are merely recognizing a very old one.

The second criticism is in substance that a quantitative comparison of the effect of the different wave-lengths on the retina is improper because the different wave-lengths constitute stimuli too different in kind to permit such comparison. This criticism we leave open, because we do not wish to discuss in this paper the propriety of the problem of comparing sensitivities.

maximum of intensity, or retinal inertia; the relative rate of fatigue to the different colors; after-image and contrast sensitivity, etc.<sup>7</sup> In fact there is not a quantitative problem dealing

<sup>7</sup>It is conceivable that two points of view may be held with regard to what is meant by after-image and contrast sensitivity. (1) After-image and contrast sensitivity may express a relation between the amount of light required to arouse after-image and contrast sensations and the unit of light used. (2) It may express a relation between the amount of light required to arouse the after-image and contrast sensations and the amount required to arouse positive sensation. If the former view should be held it will be convenient to start with stimuli equalized in energy, and to determine the relative amounts of light required to arouse the after-image or contrast sensation by means of a sectorized disc. If the second view should be held, the energy of the lights used may first be rendered proportional to the sensitivity of the eye to the colors in question; and the liminal values may then be determined by means of the sectorized disc. In each case the relative sensitivity may be expressed by the inverse ratio of the open to the closed sectors.

Similarly two views may be held with regard to the determination of the comparative rates of fatigue, and of the development-time of sensation. (1) Lights equalized in energy may be used. (2) The energy of the lights may be made inversely proportional to the sensitivity of the eye to the different colors.

The need in both the above cases is equally great for a method of regulating and determining the amounts of light to be used in terms of a common unit of measurement. For example, in the second case two ways might be conceived of making the amounts of the colored lights proportional to the eye's sensitivity to these lights. (1) The limens might be determined and the intensity of the lights always be kept directly proportional to these liminal values. But the ratio needed to maintain this proportion could not be established unless some means were available of measuring the limen-values in terms of a common unit. And if this were established, we have no right to assume that it expresses the relative sensitivity of the eye to the colors in question when greater amounts of light are used. To make this assumption, we would have to maintain (a) not only that Weber's law holds for colored as well as for white light, but also that the ratio of increase which gives the just noticeable change in intensity is the same for all colors. We do not even know that there is a constant ratio over any considerable range of the intensity scale for even a single color. (b) We would have to maintain that this ratio is the same at the limen as at greater intensities, in other words, that Weber's law holds down to the limen. The consensus of opinion among investigators is that this is not true. (2) A curve may be constructed for the particular observer in which just noticeable changes in sensation are plotted along one coordinate and the energy changes required to give these changes in sensation are plotted along the other coordinate. The subjective equation, then, would be made by choosing points on the curves for each of the colors all representing the same number of just noticeable changes in intensity of sensation from the limen. The amounts of light required to give these equally intensive sensations could then readily be read off from the energy coordinate of the curve. The energy measurements required to construct such a curve would be comparatively simple, for once the limen-value was measured in terms of energy units, the remainder of the values could be determined by means of the sectorized disc, that is, the energy change required to produce a just noticeable change in sensation is directly proportional to the ratio of change of open to closed sectors in the disc.

with the comparative functioning of the retina to the different colors in which there does not seem to be a need for the regulation and estimation of the stimulus in terms of a common unit of measurement. It is the purpose of the writers to extend the work as fast as possible into these related fields.

"(3) We wish to make a careful study of the sensitivity of the peripheral retina, quantitative<sup>8</sup> and qualitative, in a large number of meridians. In general too much uniformity has been assumed with regard to the sensitivity of the peripheral retina.

<sup>8</sup> The following are two of the points we wish to take up: (1) A determination will be made of the ratio of sensitivity of peripheral to central retina from point to point for a single color in several meridians. This will show at what rate the retina falls off in sensitivity in a single meridian, and how uniform this decrease is in the different meridians. We have found in a preliminary study that this knowledge is greatly needed in explaining certain phenomena of the peripheral retina. Furthermore, when this determination is made for each of the colors with which we wish to work, the ratios of sensitivity for these colors at all the points can be calculated and a definite answer can be given to the question whether or not uniformity of ratio obtains throughout the retina. This question has been given considerable importance in the discussion of color theories. (2) The limits of sensitivity will be investigated. In general two problems are involved here. (a) The limits may be considered in relation to the comparative sensitivity of the retina to the different colors. (b) They may be considered in relation to existing color theories. In the first of these problems the limits should be obtained with stimuli equalized in energy. So obtained the results will constitute merely another expression of the comparative sensitivity of the retina to the different colors. The second problem is more complicated and will later be made the subject of a separate paper. A word indicating its relation to our present plan of work may, however, not be out of place here. It may be logically assumed, for example, that the Hering theory demands that wherever the blue-sensing substance is found, the yellow-sensing substance must also be found. We have no means of knowing where these substances are except by the sensation aroused. Speaking in terms of the theory, then, we have a right to assume that wherever the blue sensation can be aroused the yellow sensation should be able to be aroused also, provided a sufficiently intensive stimulus be used. If, therefore, in passing towards the periphery of the retina, a point be found where blue can be aroused and yellow can not, the evidence will be strongly in favor of the conclusion that no yellow substance is present, unless it can be shown that elsewhere in the retina so much greater energy of yellow light than of blue is required to arouse sensation that the amount needed for this far peripheral point is greater than can be obtained. To establish this point the comparative sensitivity to these colors would have to be obtained at various points in the retina. This would involve the determination of a ratio based upon the amounts of blue and yellow light required to arouse sensation. Two methods of measurement may be used. (a) The amounts needed may be measured directly by means of a thermopile of the type we use, or other sensitive radiometer. In a determination of limens the number of readings required would render this method tedious. (b) The energy of the two lights may be made equal by means of a thermopile and the final amounts required to arouse sensation may be secured by means of a sectored disc. From the ratio of open to closed sectors the amount the light is cut down in each case may be calculated and the ratios of energy may be determined from these amounts.

Generalizations of great importance to color theory have frequently been based upon the results of work in which careful investigation was made in only one or two meridians. The conception of stable colors, and its application in support of the Hering *Urfarben* may be taken as a fair example of a sweeping conclusion which is based upon work too limited in its range. With a careful standardization of factors, an investigation in any considerable number of meridians shows that stable colors do not exist.<sup>9</sup> Many other points of interest have come out in our more detailed study of the peripheral retina. For example, we find in the periphery of the normal retina small areas which are exact replicas of the Schumann case of color-blindness.

"(4) We wish to conduct our investigation in full daylight instead of in the dark-room. This is to eliminate the influence of the field surrounding the colored stimulus and of the pre-exposure. When the surrounding field is black, white is induced by contrast across the stimulus color. Since the colors all differ in brightness, the induction takes place in different amounts for the different colors. This white, in proportion to its amount, reduces the action of the colors on the retina. Further, a given amount of white affects to different degrees the action of the different colors on the retina. To eliminate this twofold unequal action, the surrounding field should be made in each case of the brightness of the color to be used. This can be done by working in a light-room of constant intensity of illumination and making the surrounding field of a gray paper of the brightness of the stimulus color. In order to accomplish this, and at the same time be able to work upon any meridian of the retina we choose, we have constructed a special piece of apparatus which we call a rotary campimeter. The influence of preëxposure is even more important than of surrounding field. If the preëxposure is too black, white is added as after-image to the stimulus color. The effect of a black preëxposure upon the stimulus color is greater than the effect of a surrounding field of black, because more

<sup>9</sup> The following points are offered in support of the above statement. (1) A red and green cannot be obtained which in every meridian of the peripheral retina will pass into gray without an intermediate change into yellow or blue. (2) The amount of blue that has to be added to a mixture of red and green to produce gray varies from point to point in a given meridian even where the extramacular region alone is considered. Further, a series of determinations made for a given meridian will not hold for the remaining meridians. (3) A red, green, and yellow can not be obtained which will not change in color-tone in passing from the center to the periphery of the retina in a single meridian.

Blue alone of the four principal colors is stable in tone for all parts of the retina.

white is added as after-image of preëxposure than is induced by contrast from the surrounding field. This effect also can be eliminated only by working in a light-room of constant intensity of illumination and by choosing as preëxposure a gray of the brightness of the color to be used."

#### B. DESCRIPTION OF OPTICS-ROOM AND APPARATUS.

The work was carried on in a well-lighted optics-room,  $12\frac{1}{2} \times 10$  ft. The room is situated on the upper floor of an isolated building and is lighted by a skylight,  $8 \times 7\frac{1}{2}$  ft. Beneath the skylight, two diffusion-sashes,  $4 \times 7\frac{1}{2}$  ft. are swung on hinges so that they can be raised or lowered as desired. The framework of these sashes is made of a light-weight iron. For convenience of local control of illumination, if needed, each sash is divided into four units by means of cross-pieces. The sashes are filled with double-strength glass ground on one side, so adjusted to the frame that they can be removed easily for cleaning or for the substitution of some other kind of glass in case that is desired. This glass diffuses the light so effectively that local shadows cast by the cross-pieces in the framework of the skylight are completely eliminated, while the sudden changes of illumination produced by the passage of the sun behind a cloud are reduced to a minimum. This diffusion seems to have the further advantage of reducing the yellowness of direct sunlight below the limen of sensation. At least, when working under the sash, the observer never judged a gray exposed through the campimeter-opening as yellow under any local conditions, as frequently happened when working under direct sunlight.

The room is planned also so that small changes of illumination can be produced, ranging from the intensive illumination of a south-exposure skylight to the blackness of a moderately good dark-room. Two provisions are made for this. (a) The diffusion-sashes are made so that any or all of the panes of ground glass can be quickly and easily taken from the sash, and anything can be substituted that is desired; or the illumination can be varied by placing layers of tissue-paper above the glass. (b) The room is provided with two curtains mounted on heavy spring rollers. One is a white curtain made of thin muslin; the

other is a black light-proof curtain so mounted that, when drawn, its edges are deeply enclosed in light-proof boxing extending along the four walls of the room. One or both of these curtains can be drawn any distance that is desired, and the illumination can thus be changed gradually from a very intense brightness to a fairly good blackness. To aid in getting dark-room effects, the doors of the room are carefully boxed and curtained. One requirement of a perfect dark-room, however, is lacking, namely, the walls and floor of the room are painted white. This is because it is of advantage in the light-room work, and because complete blackness is not needed in the type of work for which the room is devised.

The apparatus used in the investigation consists of a rotary campimeter devised to meet the requirements of the task in hand by Dr. C. E. Ferree<sup>10</sup> of Bryn Mawr College. The object of this apparatus is to add to the vertical campimeter the rotary features of the perimeter and thus to allow investigation of every possible meridian of the retina with as much ease and precision as was possible with the old form of campimeter in the nasal meridian only, or at most, in the nasal and temporal meridians. The apparatus consists of two parts with proper supports and accessories; a stimulus screen, and a campimeter screen which rotates on a collar around a circular support. The stimulus is exposed through an opening in the center of the campimeter screen. One arm of the framework of this screen carries the fixation-points, and also a right-angled extension which allows fixation to be given at an excentricity of  $92^{\circ}$ . This arm may be rotated to any position desired, and thus any meridian of the retina may be explored. In order that the sensation received in the peripheral retina may be accurately expressed in terms of color- and brightness-values of the central retina, the fixation-arm of the screen is further provided with a small detachable motor upon which may be rotated the proper combination of discs for matching peripheral sensation. This increases greatly the definiteness of work on the sensitivity of the peripheral retina.

<sup>10</sup> For the original description of this apparatus, see C. E. Ferree. Description of a Rotary Campimeter. *Amer. Journ. of Psychol.*, 1912, XXIII, pp. 449-453.

The feature was added to the apparatus so that complete maps might be made of the changes in the sensitivity of the retina from center to periphery and from one meridian to another, with tables showing the value of the changes from point to point.

Photographs of the skeleton apparatus and of the front and back views of the campimeter in readiness for use are appended.

Figure 1 shows the skeleton apparatus. It consists of the following parts: supporting base, frame for campimeter screen, and frame for the stimulus card. The supporting base consists of a horizontal steel bar, 83 cm. long, supported by two iron tripod rests (B and B'). To this bar are clamped two uprights (C and C'), which are adjustable along its length. The anterior upright (C) supports the frame on which the background of cardboard and the campimeter screen (D) are fastened. The posterior upright (C') supports the stimulus frame (E). The height from the table of each of these frameworks is adjustable by means of set-screws (F and F'). The framework for the campimeter screen consists of central support and radiating arms. The central support consists of a stationary brass ring,<sup>11</sup> about which rotates a larger brass collar (H), 20 cm. in diameter. The back surface of collar (H) is graduated from 0° to 360°. To this collar are fastened the radiating arms. There are eight of these arms, one for each 45° mark of the graduated collar. They are made of steel and are 2 cm. broad and 40 cm. long. The eighth arm (I-I') differs from the other seven. It forms a right angle, one side of which is in the plane of the background and the other in front of this plane. The part in the plane of the background is 30 cm. long, and the part at right angles to this

<sup>11</sup> This ring was made large in diameter for two reasons. (a) The ring had to be made very thick in order to give sufficient rigidity to support the campimeter screen and to furnish proper attachment for the rotary collar. Had the circumference been small, the effect of the ring would have been that of a short tube. If the stimulus were viewed through a short tube, an induction factor would have been involved which would have been difficult, if not impossible, to standardize. The opening in the ring was, therefore, made considerably larger than any stimulus we wished to use in order to avoid the introduction of this factor. (b) The large circumference of the ring makes the apparatus available for investigating the effect upon sensitivity of varying the size of stimulus.

plane is 28 cm. long. The arm is graduated from  $18^{\circ}$  to  $57^{\circ}$  along the section that lies in the plane of the background and from  $57^{\circ}$  to  $92^{\circ}$  along the section at right angles. The graduations are based on the arc of a circle of 25 cm. radius. The arm is also split lengthwise to form two narrow arms, each 1 cm. wide, so separated that there is an opening (J) 0.8 cm. in width between them to admit the shank of the motor for rotating the discs needed to match the peripheral sensation. The opening to admit the shank of the motor may be clearly seen in all the pictures of the campimeter. The motor is shown at K on the right of Figure 1 and more clearly on the left of Figure 3. It has a shank 4 cm. long and 0.3 cm. in diameter, which can readily be thrust through the opening (J). The weight of the motor is so great that it can not be clamped to the arm (I-I') and thus be shifted with the arm as the retina is tested in different meridians. It has then to be supported so that it can readily and quickly be moved to any point in any meridian to which the arm (I-I') may be rotated. This is accomplished by the use of two rods—one vertical (L) and the other horizontal (M). The vertical rod (L) may be clamped to the table or other support on either side of the campimeter, and M is clamped to L. The vertical adjustment for any setting of the motor can thus be made along L and the horizontal adjustment along M. Holes are punched in each of the eight arms at six or more places to allow the insertion of small metal fasteners to hold the background screen to the frame. The stimulus frame may be seen at E. It is 20 cm. square and carries a groove for the insertion of the stimulus card. The stimulus card may be made of whatever colored paper the experimenter desires to use.

Figure 2 shows the front view of the campimeter in readiness for use; and Figure 3, the back view. A cardboard background has been fastened to the steel arms by means of paper-fasteners. Since the background is fastened to the arms attached to the brass collar (H), a circular gap is left at its center. This gap is filled by a disc (N), shown in Figure 3, which has been fastened to the arms just outside of the collar (H). The disc is 27 cm. in diameter and contains the stimulus-opening (O), the size

of which may be varied to accord with the purpose of the investigation. In the experiments reported in this paper, it was 15 mm. in diameter throughout. In order to complete the graduations on the fixation-arm to the stimulus-opening, disc (N) is graduated from  $0^{\circ}$  to  $18^{\circ}$ . A background 40 cm. in height is fastened to the extension arm (I). In the picture a paper screen made of No. 7 of the Hering series of grays has been attached by thumb tacks to the cardboard background. A strip of paper of the same quality as the background is placed along the opening (J), and the graduations from  $0^{\circ}$  to  $92^{\circ}$  are pricked on this strip as indicated by the markings on the back of disc (N) and arm (I-I'). These constitute the fixation-points. The card in the stimulus frame (E) is seen through opening (O). A disc (P) composed of black and white sectors has been placed on the motor (K).

The method of using the apparatus is as follows: The observer is seated in front of the campimeter screen with his head held in a rigid position by means of a mouthboard bearing the impression of the teeth in sealing wax. Since the graduations of the fixation-arm are based on the arc of a circle of 25 cm. radius, the distance of the eye from the stimulus-opening is chosen as 25 cm. The position of the eye in the observing plane may be obtained according to the method described by Fernald.<sup>12</sup> In order to facilitate excentric fixation in the nasal and temporal meridians, the head should be turned  $45^{\circ}$  nasalwards or temporalwards, as the case may be. With the head so placed, the eye can swing easily from the stimulus-opening to a fixation-point whose excentricity exceeds  $90^{\circ}$ . The unused eye is closed and covered by a bandage. The arm (I-I') is placed in the meridian desired, the position being indicated by the graduations on the collar (H). The experimenter covers the stimulus in the stimulus frame with a card, which we shall call the preëxposure card, while the observer takes the fixation required. At a signal given by the observer, the preëxposure card is withdrawn, the stimulus is exposed for three seconds, and the preëxposure card is replaced

<sup>12</sup> Fernald, G. M. The Effect of Achromatic Conditions on the Color Phenomena of Peripheral Vision. *Psychol. Rev.*, Monograph Supplements, 1909, X., p. 18.

over the stimulus. The observer is required to rest the eye after each observation. Further provisions against fatigue are made by periods of rest after each fifteen minutes of observation.

When it is desired to measure the stimulus as seen in the peripheral retina in terms of brightness- and color-values of the central retina, the motor shown at K in Figures 1 and 3 is used. The method of making the measurement is as follows: If a direct vision judgment, for example, of the appearance of yellow at  $25^\circ$  in the temporal meridian is wanted, the cord (R) carrying a movable fixation-point, seen in Figure 2, is fastened in front of the  $25^\circ$  point on the graduated background. The observer, in position, fixates the  $25^\circ$  point and brings the movable point in line with the eye and the  $25^\circ$  point. This point then serves as the new fixation-point, and the graduated strip covering the opening (J) is removed. The required discs are placed on the motor immediately behind the new fixation-point, and their proportions are changed until the observer judges that the sensation aroused in the periphery is matched by that aroused in the center by the measuring-disc on the motor. In making this judgment, the method of ascending and descending series was used.

In this investigation, stimuli of blue, yellow, red, and green pigment papers of the Hering series were employed. White, black, and gray papers of the Hering series served to make the backgrounds. Results were obtained from three observers: Miss Campbell, *C*, graduate student in Bryn Mawr College, who had no knowledge of the problem in hand, Dr. Ferre, *B*, and the writer, *A*.

#### C. DETERMINATION OF THE BRIGHTNESS OF THE COLORED STIMULI EMPLOYED IN THE INVESTIGATION.

At every turn in our problem, it was necessary to know the black-white-values of the colored papers that formed our stimuli, as they appeared in the central and peripheral retina at full and decreased illumination. It was thought best, therefore, to devote a separate chapter of our report to a discussion of the methods used in determining these values. The method of flicker photometry was used throughout except at the limit of peripheral color vision, where it was possible to use the method of direct com-

parison. The black-white-values of the colors were determined for the central retina by means of the Schenck *Flimmer Photometer*. As this apparatus is not adapted to indirect vision work, it was necessary to devise a means by which the brightness of the stimuli at any point in the peripheral retina could be determined by the flicker method. The conditions of our experiment made it essential that these determinations be made not only in terms of black-white-value but also of colorless pigment paper, the brightness of which would approximately be the same as that of the colored stimulus. In order to make the latter determination possible, a series of gray papers varying in brightness by very small amounts was required. The Hering papers, ranging in number from 1 to 50, were found to furnish a series which varied in brightness by amounts sufficiently small to serve our purpose.

The use of the flicker method in photometry is based on the fact that two surfaces are considered equal in brightness when upon their alternation one with the other at a certain favorable rate of speed, no experience of flicker results. Obviously a very important point in the method is to determine what this rate of alternation should be. It should be determined empirically for each observer in a preliminary experiment. To make the determination we must be able to produce known brightness differences in different parts of the scale and to try the effect on flicker of different rates of speed for these brightness differences. This can be accomplished by making the preliminary experiment with colorless surfaces, for very small differences in brightness between two colorless surfaces can be estimated by the method of direct comparison. (This could not be done if one or both of the surfaces were colored.) Working then with colorless surfaces by the aid of the method of direct comparison, not only do we know at every stage of the experiment how much brightness difference is produced, but we standardize the flicker determination in terms of the method of direct comparison to which all indirect methods of determining brightness equality must conform if their results are to be of any value. In making our preliminary determination, then, colorless surfaces should be used and that rate of alternation, equal to or in excess of the

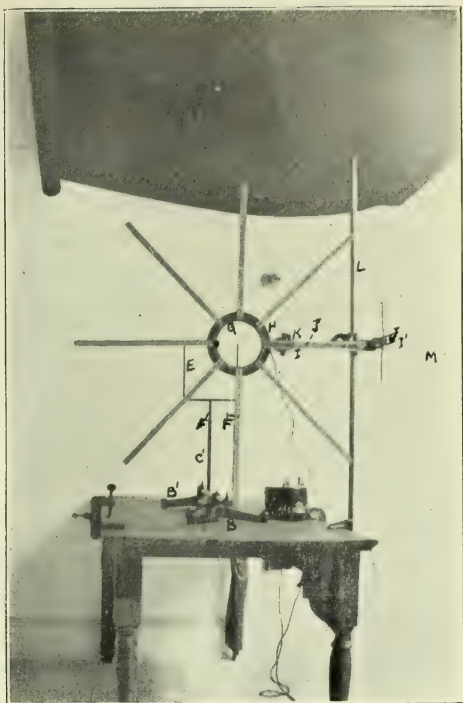


FIGURE I

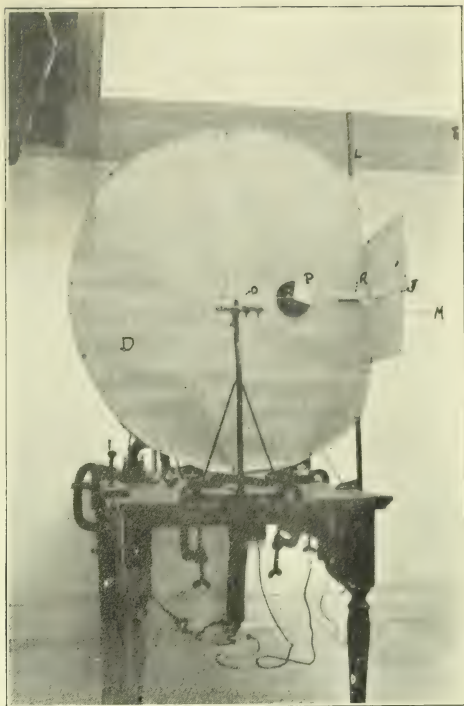


FIGURE II

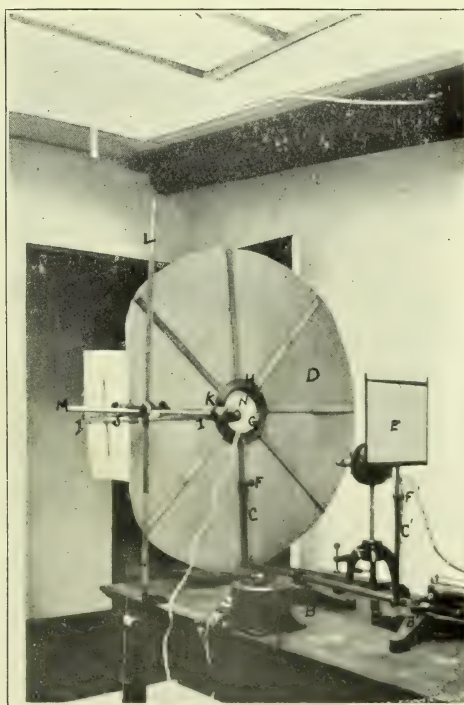


FIGURE III



fusion rate for the color in question, should be selected at which the smallest difference in brightness between the two surfaces produces flicker. This speed may be considered as giving the maximum sensitivity to the method for the given observer and may be used for that observer in the color work. Applying this method to the determination of the brightness of a colored paper, we may consider that a colored and a gray paper are of equal brightness when no flicker is produced by the rotation of equal sectors of each at the chosen rate of speed. In order to prevent induction from the surrounding field, the rotating disc should be viewed through an opening in a screen of the same brightness-value as the disc itself. This requires that the gray sector, the colored sector, and the screen all be of equal brightness-value. The final value of this brightness must, however, be reached by a series of approximations. That is, the gray sector and the screen must at each trial be chosen of the same brightness, and both must be changed alike until a gray is finally obtained which does not produce flicker when it is rotated with the colored sector at the chosen rate of speed. Owing to the great number of steps in the series of approximations needed to reach the gray-value of the color, it was impracticable to change the large campimeter screen at each step. Moreover, to prevent brightness induction over the stimulus, it was not necessary to have so large a part of the surrounding field, as was comprised in the entire screen, of the same brightness as the stimulus. Squares 30 cm. on a side were found to be quite adequate for the purpose and to have the practical advantage that they could be quickly and easily changed. Since the sensitivity of the retina to flicker varies from point to point as we pass from the fovea outwards, it is found to be important that the opening in the screen be made small so that the area of the retina stimulated shall be uniformly sensitive to flicker. And, further, since the area of the retina influenced by a given stimulus decreases as we pass from the fovea to the periphery, because of decrease in the visual angle, it is necessary that the stimulus-opening be proportionately increased in the peripheral observations, in order to maintain the size of the stimulated area of the retina constant. Both of these conditions were met sufficiently accurately for our purpose, by using two

openings of different size, the smaller to be employed at all points from the fovea to  $20^\circ$  peripheralwards, and the larger to be used from the  $20^\circ$  point to the extreme periphery.

The method used to determine the brightness in terms of gray paper of a given color in central vision was as follows. From the series of squares of gray papers having the smaller of the two sizes of stimulus-openings (3 mm.  $\times$  1 mm.) one was selected which was judged by the method of direct comparison roughly to approximate the brightness of the color in question. This square was fastened upon the campimeter screen so that the stimulus-opening passed vertically through the center of the opening in the original screen. A disc compounded of  $180^\circ$  of the given color and  $180^\circ$  of the gray of the brightness of the square, was rotated behind the stimulus-opening and the observation made for flicker. Lighter and darker grays were in turn substituted in disc and screen and the observation was repeated. The gray which produced no flicker at the chosen rate of speed is, in terms of the method, the gray of the brightness of the color. The determinations were not at all difficult nor uncertain. Flicker was readily discernible in the gray lighter and darker than the one which was chosen, at the speed of rotation at which the one chosen showed no flicker. Our determinations showed that at the standard illumination<sup>13</sup> used throughout the work, the method of obtaining which will be discussed later, the brightness of Hering blue for central vision equalled that of Hering gray No. 41; of red equalled gray No. 24; of green equalled gray No. 8; and of yellow equalled gray No. 2. These values were the same for all of our observers.

For the determination of the brightness of the colored stimuli in peripheral vision, the same method was used with the exception that at points in the periphery beyond  $20^\circ$ , the screens having the large stimulus-openings, 15 mm. in diameter, were used. The results of the peripheral experiments differed from the central at standard illumination only in case of blue. Blue was found to lighten in the periphery so much that gray No. 21 was determined

<sup>13</sup> Measured in foot-candles by means of the Sharpe-Millar portable photometer, the standard illumination equalled 390 foot-candles.

by one observer to equal it in brightness. The determinations at the peripheral limit of sensitivity to color were made by the method of direct comparison. On the campimeter was mounted a gray screen for each of the colors in turn the brightness of which was such that when the stimulus was observed beyond the limits of color sensitivity, the color in each case changed into the gray of the brightness of the screen. If a gray lighter or darker was used, the stimulus appeared either darker or lighter than the screen.

The black-white-values of the colors for peripheral vision can not be determined directly because for the direct determination the Schenck *Flimmer Photometer* with its graduated sectors of black and white or some similar device must be used. This photometer is not adapted for peripheral vision work. A determination, then, had to be made with the photometer in the central retina for the grays which had been found by the method described in the preceding paragraph to equal the colors in brightness in the peripheral retina.

In one section of the investigation, it was found necessary to work at decreased illumination, and to know the brightness of the colored stimuli under these conditions. The decreased illumination was obtained by drawing the black curtains, until the illumination was slightly less than that of a cloudy afternoon.<sup>14</sup> The colors appeared a little less saturated, and slightly altered in color tone. The green was a trifle bluish, the yellow was changed toward orange, and the blue appeared slightly reddish. Determinations were made at this illumination according to the method described above, of the brightness of the colors in central and in peripheral retina. The results are stated in Tables I and II. The first column shows the white-black-values of the stimuli at standard illumination in central vision; the second shows the same values in terms of the Hering gray papers; the third represents the brightness-values in peripheral vision at the limit of sensitivity; and the fourth and fifth columns show the brightness of each stimulus at the center and at the periphery under conditions of decreased illumination.

<sup>14</sup> Measured in foot-candles by means of the Sharpe-Millar portable photometer, this decreased illumination equalled 1.65 foot-candles.

Table I records the results of Observer *A*; Table II those of Observer *C*.

TABLE I.

*A. Showing the brightness-values of the Hering principal colors at standard and decreased illumination.*

Stimulus	Brightness at Standard Illumination			Brightness at Decreased Illumination	
	At center		At limit of sensitivity in periphery	At center	At limit of sensitivity in periphery
Yellow	white 236° black 124°	gray no. 2	gray no. 2	gray no. 2	gray no. 3
Green	white 100° black 260°	gray no. 8	gray no. 8	gray no. 8	gray no. 5
Red	white 41° black 319°	gray no. 24	gray no. 24	gray no. 24	gray no. 50
Blue	white 15° black 345°	gray no. 41	gray no. 28	gray no. 35	gray no. 13

TABLE II.  
*Observer C.*

Yellow	white 236° black 124°	gray no. 2.	gray no. 2	gray no. 2	gray no. 3
Green	white 100° black 260°	gray no. 8	gray no. 7	gray no. 7	gray no. 4
Red	white 41° black 319°	gray no. 24	gray no. 24	gray no. 24	gray no. 50
Blue	white 15° black 345°	gray no. 41	gray no. 21	gray no. 35	gray no. 7

Whether or not the peripheral retina functions differently from the central retina and must, therefore, be assumed to possess a different sensory mechanism, is a question of considerable importance to theories of color vision. Upon this question the results shown in Tables I and II have a direct systematic bearing. But since the comparative functioning of central and peripheral retina will be made the subject of a later report of work already completed by the writer, the significance of these results need not detain us here. We need only note in passing,

that the brightness changes that occur when a stimulus is carried from central to peripheral vision are similar to those that obtain in central vision when the illumination is decreased. With regard to this point our tables show (*a*) that in the peripheral retina at standard illumination, the colors have very nearly the same brightness relations that they have in the center at the decreased illumination we used; and (*b*) that the brightness relations of the colors seen in the peripheral retina at decreased illumination approximate those of the colors in the center when the illumination is further decreased. These latter changes known as the Purkinje phenomenon are in the following directions: blue and green relatively lighten; red and yellow relatively darken.

In Tables I and II, showing the comparative brightnesses of the color sensations at center and periphery, the lightening of blue and green and the darkening of red in the periphery are sufficiently pronounced to need no comment. But measured by these results, the change in yellow seems to be insignificant. If, however, yellow is observed in the periphery at decreased illumination and is compared with gray No. 2, that is, the brightness of yellow both in center and periphery at standard illumination and in center at decreased, it appears to be much darker than the gray screen. Contrast from the screen exaggerates this darkening to some extent but the change in the brightness of the sensation due to peripheral stimulation alone is considerable.

#### D. THE FACTORS INVESTIGATED.

The factors we have investigated with regard to their influence upon the color observation are: (1) the brightness of the stimulus; (2) the brightness of the field surrounding the stimulus; (3) the brightness of the preëxposure; and (4) the general illumination of the retina.

##### 1. *Brightness of the Stimulus.*

It will be remembered from the historical discussion (p. 45 ff.) that the four men,—Bull, Hegg, Hess, and Baird,—who recognized the need of equating the intensity of colored stimuli for a determination of the relative limits of color sensitivity, equated them also in brightness. They apparently assumed the need of

this equation without having investigated the influence of the brightness difference between the colors upon the breadth of the color zones. As a result of a careful investigation of this question, we are able to show that not only is no advantage gained by equating the brightness of colors when determining their limits of sensitivity, but a positive disadvantage is suffered. The following reasons may be cited in support of the latter statement. (a) The quality of certain colors is changed when their brightness is altered. This disadvantage, first mentioned by Chodin, was a source of great difficulty to Hegg, as we have seen (p. 74).<sup>15</sup> (b) Colored stimuli which have been equated in brightness are necessarily reduced in intensity. For this reason, no true nor comprehensive estimate of the color sensitivity of the retina can be obtained with stimuli equated in brightness. (c) The technique involved is extremely cumbersome.

Our investigation of the influence which the brightness difference between the four principal colors exerts upon their limits covers four points. (a) The work on the first took its start from Hegg.<sup>16</sup> Hegg apparently assumed that the difference in the brightness of the four colors,—red, green, blue, and yellow,—at full saturation is sufficient to affect their limits, and, therefore, that they must be equated in brightness before a determination of their relative limits is made possible. With the view of testing the validity of this assumption, we sought to ascertain whether a brightness change in any one of the four colors, equal to the maximal brightness alteration made in Hegg's equation, affects the limits of sensitivity to that color, provided the alteration is produced without changing the amount of colored light coming to the eye. The test was made doubly strict by varying the colors both toward white and toward black, thus covering a variation whose range was twice as great as was required. That is, both white and black in turn were added to each of the colors in

<sup>15</sup> Mrs. Franklin remarks concerning Hegg's oil papers: "Of the 'normal' colors prepared by Hegg, the red and the yellow would not strike the plain man as at all deserving of the name" (*Psychol. Rev.*, 1897, IV., p. 96).

<sup>16</sup> A start was taken from Hegg, in preference to the other three investigators, because he is the only one who gives adequate numerical data concerning the extent of brightness alteration he made in obtaining this equation.

amounts equal to Hegg's maximal change, and the limits of the stimuli thus obtained were compared with each other, and with the limit of a stimulus equal to them in physical intensity and to the original color in brightness. No effect whatever on the limits was found as a result of these brightness alterations. (b) We next sought to ascertain whether a brightness equation is necessary when working with the standard pigment papers of the Hering series. A determination of the maximal brightness difference between the colors was made at the limit of sensitivity to each, and the above experiments were repeated using the maximal value obtained in these determinations, as the amount of variation. In no case were the limits affected. (c) Following Hegg's plan of equating the four colors to one of mid-brightness, we next determined the maximal amount of change required to equate the standard Hering colors to the brightness of green. Since the amount of change was obviously much less than the variations used in (b), it was not necessary to repeat the experiments on the limits of sensitivity with this amount of variation. In two ways, then, we shall have shown that it is unnecessary to equate the Hering colors for brightness when determining their relative limits of sensitivity, since neither the maximal amount of change required to bring them all to a medium brightness, nor the maximal amount of brightness difference between the colors, has any effect upon the color limit when this change is applied as a variant in the direction of either white or black, provided that the amount of colored light coming to the eye remains unaltered throughout. These results may seem contradictory to the statement made by certain other writers and by ourselves<sup>17</sup> that dark colors appear more saturated than light colors of equal physical intensity, that is, white exerts a greater inhibitive action than black upon color. This brings us to our fourth point. (d) We have to explain why these brightness changes which are known in general to affect the sensitivity of

<sup>17</sup>Ferree, C. E. and Rand, G. Colored After-image and Contrast Sensations from Stimuli in Which No Color Is Sensed. *Psychol. Rev.*, 1912, XIX., pp. 215; An Experimental Study of the Fusion of Colored and Colorless Light Sensation: The Locus of the Action. *Journ. of Philos. Psychol. and Scientific Methods*, 1911, VIII., pp. 294-297.

the retina to color do not change the limits of color. The explanation, as will be shown on p. 104, is found in the extreme rapidity with which color sensitivity of the retina falls off near the limits. The amounts of brightness dealt with in the above cases do not produce a sufficient change in the saturations of the light and the dark color to cause their limits to differ by even  $1^\circ$ , because the stimuli reduced in intensity by this amount of brightness are still sufficiently intensive to cause the color limits to occur within the zone of rapid decrease in sensitivity. If the stimuli had given a very small amount of colored light to the eye, and the limit of sensitivity had consequently occurred nearer the center of the retina where the sensitivity falls off more gradually, the difference in saturation between a dark and a light color of equal physical intensity, might have been sufficient to cause the latter to have a narrower extent of visibility. But since the amount of intensity at which this exception occurs is much less than is ever likely to be used in investigating the color limits, the exception can scarcely be entitled to more than theoretical consideration. The writer regrets to report that she has not carried on this investigation with spectral light. While she has no reason for believing that the results would in general be different, still for the sake of knowing the exact values of the brightness quantities obtaining in case of spectral colors, she hopes to make the investigation in the near future.

To investigate these points, it was necessary to devise a method whereby the brightness of the color can be altered without changing the amount of colored light coming to the eye. When one is working with pigment papers, the brightness of the stimuli can easily be varied without changing the intensity of the stimulus. For example, discs can be compounded of  $260^\circ$  of yellow and  $100^\circ$  of white,  $260^\circ$  of yellow and  $100^\circ$  of black,  $260^\circ$  of yellow and  $100^\circ$  of the gray of the brightness of yellow. In these cases we have, in order, a tint of yellow, a shade of yellow, and a yellow reduced in saturation but not changed in brightness, —all giving the same amount of yellow light to the eye. If it should be desired to make the tints darker and the shades lighter, the brightness sectors can be chosen of white or black in any proportion that is required.

In determining the size of the brightness sector to be used for the first point in this investigation, we are obliged to proceed largely by inference from Hegg's rather meager report of his work. He had equated the peripheral brightness-values of his four stimuli to green. In doing this,  $84^\circ$  of white and  $5^\circ$  of black were added to red. No statement whatever is made by him with regard to the amount of white and black added to blue and yellow. These amounts have, therefore, to be inferred. In doing so, care was taken to make the amount sufficiently large to give our test due rigor. We have mentioned that blue lightens in the periphery until its brightness is much like that of red. For Observer *A* it is slightly darker than red, for *C* slightly lighter. It is fair, then, to assume that  $100^\circ$  represents the maximal brightness difference that Hegg found to obtain between the colors in their peripheral values. Observations were taken from sets of discs composed of sectors of  $260^\circ$  of each of the four principal colors and  $100^\circ$  in turn of white, of black, and of the gray of the brightness of the color. The surrounding field and preëxposure in each case were of the gray into which the stimulus color changed at the limit of sensitivity. Several observers were used and several meridians explored, but in no case could a difference in the limits of sensitivity be detected for the color mixed with white for the color mixed with black, and for the color mixed with the gray of the brightness of the color. Space will not be taken here to record the results for all observers in all meridians investigated. The results obtained for Observer *A* for the temporal and nasal meridians are selected as typical. They are shown in Table III.

We have seen that the alteration made by Hegg in his brightness equations, when applied to stimuli of Hering standard papers, does not affect their limits of sensitivity. We now pass to our second point, namely, whether the full brightness difference in these colored papers should be considered as in any way affecting their limits. This is somewhat different from the preceding point in which we were concerned merely to find out whether Hegg's attempt to reduce all the colors to a mid-brightness could be considered as having any effect upon their limits.

TABLE III.

A. *Showing the limits of sensitivity<sup>18</sup> when the colors are mixed in turn with 100° of gray of the brightness of color, 100° of white, and 100° of black without altering the amount of colored light coming to the eye.*

Stimulus	Meridian	Limit of stimulus when mixed with 100° gray	Limit of stimulus when mixed with 100° white	Limit of stimulus when mixed with 100° black
Yellow	Temporal	42°	42°	42°
Green		35°	35°	35°
Red		41°	41°	41°
Blue		51°	51°	51°
Yellow	Nasal	88°	88°	88°
Green		59°	59°	59°
Red		85°	85°	85°
Blue		91°	91°	91°

We wish here to find out whether the actual difference in brightness between the extreme members of the series, blue and yellow, affects the limits of any one of the series. To do this, our method was to determine the difference between blue and yellow at their limits of sensitivity, to vary each color toward both white and black by the amount of this difference, and to find out whether the limits of the light and the dark stimulus differ from each other or from that of a stimulus of equal intensity which has retained the original brightness of the color. This amount of variation was greater than was needed in case of red and green, because they do not differ from any member of the series by so great an amount. We have used this maximal amount, however, because we have not wished to leave room for any question as to the rigor of our test.

In order to ascertain the difference between the white-values of the colors seen in the extreme periphery, the Hering gray that represented the peripheral brightness of each stimulus as determined by the method of direct comparison at the limit of sensitivity (see Tables I and II), was mounted on the Schenck

<sup>18</sup> The point at which color loses all trace of its original quality is recorded as the limit of sensitivity.

*Flimmer Photometer*, and its white-value determined. For Observer *A*, blue was the darkest color. Its brightness was equal to white  $37^\circ$ , black  $323^\circ$ ; that of red was equal to white  $41^\circ$ , black  $319^\circ$ ; that of green was equal to white  $100^\circ$ , black  $260^\circ$ ; that of yellow, the lightest color, was equal to white  $236^\circ$ , black  $124^\circ$ . The maximal brightness difference, then, was between blue and yellow, and was equal to  $199^\circ$ . To ascertain whether this brightness difference is sufficiently great to influence the breadth of the color zones, the limits of stimuli composed of  $161^\circ$  of each color and  $199^\circ$  of black, and of  $161^\circ$  of color and  $199^\circ$  of white were compared with each other and with the limit of a stimulus composed of  $161^\circ$  of color and  $199^\circ$  of gray of the brightness of the color. The first two stimuli, it will be observed, were composed of the color altered in brightness toward black and toward white by an amount equal to the difference in white-value between blue and yellow; the third stimulus retained the original brightness of the color while it sent the same amount of colored light to the eye as the other two. In every case, on either the nasal or the temporal meridian, the limit of color visibility was the same whether the stimulus was the color in its original brightness or whether its brightness was changed in either direction, toward black or toward white, by an amount equal to the maximal difference between the white-values of the colors as seen in peripheral vision.

As we have said, our test is unnecessarily severe. Not only have we lightened blue and darkened yellow by an amount equal to the difference in their white-values, but we have also darkened blue and lightened yellow by the same amount. If a brightness equation were found to be necessary, the variation would by no means be as wide as the one we have made. It would be necessary merely to darken some colors and to lighten others to a medium brightness.<sup>19</sup> We feel confident then in stating the following

<sup>19</sup> The change we have made in one direction is no greater than had to be made by Baird who equated all of his colors to the brightness of blue. Baird, it will be remembered, was forced to employ this brightness as standard because his equation of brightness was made by interposing an episcotister between the stimulus and the eye of the observer. This method permitted change only in one direction, towards black. The defects of his method

two points:—(1) The amount of change required to equate in brightness the colors, red, green, blue, and yellow, has no effect upon their color limits and the precaution of equating is, therefore, superfluous. (2) The actual brightness difference in the colors at standard saturation has no effect upon their relative limits.

While we have shown that variations of brightness in the above amounts do not affect the limits when there is no alteration in intensity of the colored light, we do not claim that there might not be a change sufficiently large to influence the limits. This would be a broader thesis than we wish to maintain. We have merely been concerned with showing that brightness alterations as great as the difference between the white-values of the Hering standard papers do not affect the limits. Strictly speaking, this is as far as our criticism of previous attempts to standardize brightness need carry us. But it is a matter of fact that a color mixed with black gives us a sensation that is more intensive than that produced by a color of equal physical intensity which is mixed with white, and that the limen of color is much lower when the color is mixed with black than when mixed with white. Brightness change, then, does affect the retina's sensitivity to color, and, within limits, the breadth of the zones of sensitivity. We have, therefore, extended our investigation to explain why changes of the order given above do not affect the color limits, and to determine roughly to what extent brightness change may be made without affecting them. As already indicated we must look for the explanation of our results to the rapidity with which the sensitivity of the retina falls off from point to point from center to periphery. If, for example, it be found that sensitivity falls off gradually from the fovea to near the limits (as determined with stimuli of full intensity,) and from that point on, it falls off abruptly, we might expect that light and dark colors of equal physical intensity will have different limits up to the point on the retina at which the abrupt change has already been pointed out. With a spectroscopic mixer as the ideal apparatus for investigations with the light of the spectrum, the brightness changes can be readily made in both directions, as they can with pigment paper stimuli.

change in sensitivity begins, and the same limits from that point on. It is obvious that in either case, whether or not there is a difference in limit, depends upon whether the difference in the inhibitive action of white and black upon the color is equal to the amount of change of intensity required to affect the limit. If sensitivity falls off gradually, a relatively small change in intensity is sufficient to widen the limit, and, if abruptly, a relatively large amount of change is required.

By way of explanation, it is our purpose to show (*a*) that the sensitivity of the retina falls off gradually to a point within  $5^\circ$  of the limit and from that point to the limit, it falls off very abruptly; (*b*) that the white and black sectors added to the colored stimuli in the foregoing tests did not weaken the stimuli sufficiently to narrow their limits more than  $3^\circ$ ; and (*c*) that within the zone  $3^\circ$  from the limit, the difference between the apparent saturations of our light and dark stimuli was not sufficient to affect their limits.

An inspection of the results given in Table VIII and discussed in the next section (p. 117 ff.) will show the rate at which the sensitivity of the retina falls off from the fovea to the periphery and will establish our first point. The decrease is gradual from the center to within  $5^\circ$  of the limit, beyond which point it grows progressively more abrupt, becoming extremely abrupt from a point  $3^\circ$  from the limit to the limit. For example, when the screen and preexposure of the gray of the brightness of color are used, the limen of yellow at the fovea is  $18^\circ$ ; at  $39^\circ$  from the fovea in the temporal meridian, that is,  $5^\circ$  from the limit of yellow, it is  $100^\circ$ . Thus over a space of  $39^\circ$ , the limen has increased only  $82^\circ$ , and average of little more than  $2^\circ$  of increase per degree of retina traversed. At  $41^\circ$ , however, it has reached a value of  $150^\circ$ , an average of  $34^\circ$  of increase per degree of retina traversed; at  $42^\circ$ , a value of  $240^\circ$ , an average of  $90^\circ$  of increase per degree of retina traversed; at  $43^\circ$ , a value of  $330^\circ$ , an average also of  $90^\circ$  per degree of retina traversed. With regard to the second point, it will be remembered that the extreme amount of white or black we added to our colors was  $199^\circ$ . This left  $161^\circ$  of color in the stimulus discs. Table VIII

(page 119) which gives the values of the color limens at different points near the limit, shows that this amount of color is above the limen for each color at  $3^\circ$  from the limit. In our tests, then, we were working well within the  $5^\circ$  limit bounding the zone of abrupt decrease in sensitivity, as our explanation required us to show. With regard to the third point, it will be seen from the same table that, when working at the point  $3^\circ$  within the limit, in order to extend the limit  $1^\circ$ , an increase of the colored sector by amounts ranging from  $65^\circ$  in the case of blue to  $115^\circ$  in the case of green, is required. It scarcely need be pointed out that the apparent saturation of a stimulus composed of  $161^\circ$  of color and  $199^\circ$  of black is not greater than the apparent saturation of a stimulus composed of  $161^\circ$  of color and  $199^\circ$  of white by an amount equivalent to from  $65^\circ$  to  $115^\circ$  of color.

Having explained why brightness differences equal to those found in red, green, blue, and yellow papers of standard saturation have no effect upon the limits of color sensitivity, we turn next to a determination of the range within which brightness change may be made without affecting the limits of sensitivity. Two ways occur to us by means of which a rough estimate of this range may be obtained. (a) Stimulus colors at full saturation may be used and the brightness excitation be added as after-image or contrast or both. In this way the amount of colored light coming to the eye is not altered by the brightness added, that is, the physical intensity of the color in the stimulus is not affected. If we wish to use the contrast and after-image effects, the card which covers the stimulus before exposure can be adjusted so that an intensive after-image is superimposed upon the stimulus when the card is removed. By a proper regulation of this card and of the campimeter screen, which causes contrast induction across the stimulus, varying amounts of white and black can be added to the stimulus, care being taken to measure these amounts and to keep them equal, each to each. Since, according to our measurements in this region of the retina, the after-image and the contrast excitations from white are more intensive than those from black, the quality of the screen and

preexposure designed to give dark contrast must be regulated until the brightness excitation aroused is found to be equal in amount to the white given by the black screen and preexposure. A series of these changes can be made until a point is reached where the sensations are reduced in intensity sufficiently to allow the more saturated dark color to be seen farther out than the light color. The sum, then, of the amounts of white and black added in turn to the stimulus, will give the range of brightness change that may be made in a stimulus of full intensity without causing the difference in brightness to be a factor influencing color limits. (b) Equal sectors of white and black may be added to the stimulus color until a point is reached where the darkened color is seen farther out than the lightened. This method has the disadvantage that with each addition to the brightness sector, there is a corresponding subtraction from the color sector. On the other hand, however, it may have a possible advantage over the former method in that the brightness excitation that is added to the color is aroused by light-waves, as is the case with the standard colors whose brightness differences gave rise to our problem; hence any theoretical questioning is obviated as to the quantitative equivalence of the action of a brightness excitation objectively aroused to an excitation aroused as after-image or contrast. But since we can not work with colors at full saturation, the disadvantage is probably much in excess of the advantage. We can doubtless come much closer to the value we are seeking by the first method. As the work by this method is not completed, its report will be deferred until a later paper. The results obtained by the second method are given in Table IV. In this table we have shown how much the colored sector may be reduced by the addition of black and white, without changing the limits for the darkened and the lightened color. If a further reduction is made, the darkened color will be seen at a greater excentricity than the lightened color. The results show that  $240^\circ$  of black, white, or gray of the brightness of color may be added to yellow and the limits for the three shades of color so formed will still coincide;  $225^\circ$  to red;  $215^\circ$  to blue; and  $230^\circ$  to green. Since, roughly speaking, the amount of inhibition will be inversely proportional to the amount of color

present, it is obvious that if the colors could have been maintained at full intensity, as they usually are in the investigation of sensitivity, a still greater brightness change would have been possible. While we may not have determined by this method just how much brightness difference there may be between colors at full saturation without affecting the limits, we have shown beyond doubt that there may be much more than is found between the standard pigment colors.

Table IV gives some of the results of this investigation for Observer *A* in the temporal meridian. Each observation was taken with screen and preëxposure card of a gray of the brightness of the color. Since the results in the nasal meridians are very similar to these, space will not be taken to report them. As the sensitivity of the retina falls off gradually in all directions until within  $5^\circ$  of the limit, the limen at this  $5^\circ$  point is almost identical, whatever the meridian.

TABLE IV.

*A. Showing how much white, black or gray of the brightness of the color we may add to a colored stimulus and still have a coincidence of limits for the three shades of color, providing the amount of colored light coming to the eye is kept constant.*

Stimulus	Value of colored sector	Value of brightness sector (gray of brightness of color, white, or black)	Limit of sensitivity when color is mixed with gray of brightness of color	Limit of sensitivity when color is mixed with black	Limit of sensitivity when color is mixed with white
Yellow	260°	100°	42°	42°	42°
	180°	180°	40°	40°	40°
	90°	270°	37°	38°	37°
	120°	240°	40°	40°	40°
	105°	255°	39°	40°	39°
Green	120°	240°	30°	31°	30°
	130°	230°	31°	31°	31°
Red	120°	240°	37°	38°	35°
	135°	225°	39°	39°	39°
Blue	135°	225°	48°	48°	43° <sup>20</sup>
	145°	215°	49°	49°	49°

<sup>20</sup> The decided narrowing of the limit of the blue stimulus in this case

But there is more than one kind of problem which deals with peripheral color sensitivity. To avoid any possible misunderstanding of our position, a word may be added to show when it is of advantage and when of disadvantage to equate stimuli in brightness. (a) When investigating the limits of color sensitivity and when the brightness of the surrounding field is the same as the brightness of the stimulus color, a brightness equation of the different colors, within the limits we have just determined, is not only unnecessary, but a positive harm. This, moreover, is the proper regulation of the brightness of the surrounding field for all investigations of the relative and absolute limits of sensitivity and of the limens of color at different points on the retina. (b) When, however, the brightness of the surrounding field is different from that of the color, the factor of the induction of the screen must be taken into account. Since brightness contrast follows the law that maximal contrast occurs when there is a maximal brightness opposition, different amounts of contrast will be induced across colors of different brightnesses. But under these conditions, only one legitimate problem can arise, namely, to test the effect of the screen. There are two points to this problem. (i) Knowledge of the effect of different screens upon the same color may be desired. In this case, the problem of

is due to the following cause. For Observer *A* there is a small spot in the horizontal temporal region of the right eye that is totally insensitive to blue light. This miniature spot of blue-blindness extends from  $43^\circ$  to  $47^\circ$  in the horizontal temporal meridian. Now since the apparent intensity of the sensation aroused by the stimulus composed of  $135^\circ$  of blue and  $235^\circ$  of white was not sufficient to allow the color to be seen on the peripheral side of this blue-blind spot, its limit occurred on the foveal limit of the spot, at  $43^\circ$ . It may be added that spots of this type are not unusual. The writer has found in every eye she has tested one or more spots that are partially or totally insensitive to one color alone. Relative to these blind spots, the following interesting features may be noted. (a) Although totally blind to a given color, they have normal sensitivity to its complementary color. (b) They give a fully saturated complementary-colored after-image of this color to which they are blind. (c) They show the usual cancelling action between the color to which they are blind and its antagonistic color. In short, they seem to be exact replicas in the periphery of the normal eye of the unique type of color-blindness described by Schumann (see Schumann, F. *Ein ungewöhnlicher Fall von Farbenblindheit. Bericht über die 1. und 2. Kongress für experimentelle Psychologie*, 1904, pp. 10-13.

brightness equation would not arise. (ii) Knowledge of the effect of the same screen on different colors, or of the comparative effect of different screens on more than one color may be desired. In this case the colors may or may not be equated in brightness:—the question depending upon the requirements of the problem. If they are not equated in brightness, there will be different amounts of induction with each screen for each color. If they are, the colors will be altered in intensity and often in color tone. No general rule can be laid down as to equation or non-equation in these cases. Each has to be settled on its own merits and in accord with the requirements of the problem in hand. What we wish to emphasize more than anything else at this point is that, while at different times in color work, one may need to make legitimate use of a surrounding field which differs in brightness from the stimulus color, it should never be done in any investigation of the relative or absolute limits or limens of color sensitivity. The use of the perimeter and the dark-room is a notable instance of the violation of this precaution. The surrounding field of intensive blackness induces a different amount of white over each of the colors unless they are of the same brightness. And if they are equated in brightness, all the disadvantages which, as pointed out earlier in the paper, result from this equation, are suffered in the investigation. Moreover, to equate the stimuli in brightness is not to get rid of the induction of the surrounding field. We still have, after equating, a large amount of brightness induction which operates against a determination of absolute limits by tending to narrow the limits of sensitivity for all colors; and against a determination of relative limits by narrowing the different colors unequally, depending upon the difference in the inhibitive action of the same amount of white upon them.

## 2. *Brightness of the Field Surrounding the Stimulus.*

When a small color stimulus is surrounded by a large field of white or black, a sensation is given which consists of the color mixed with black or white, due to contrast induction from the surrounding field. The influence of the brightness of the surrounding field upon color sensitivity resolves itself, then, into the

question of the fusion of colored with colorless light sensation in central or peripheral vision, according to the part of the retina that is stimulated. The details of this fusion in central vision have been taken up by the writer working in collaboration with Dr. C. E. Ferree,<sup>21</sup> in which work it was shown that the effect of fusing a colored sensation with white, black, or gray is twofold. (a) There is a quantitative effect due to the inhibition of chromatic excitation by achromatic. White inhibits color most, the grays in order from light to dark next, and black the least. The records of all the observers used in this investigation show that the achromatic series inhibits red and yellow considerably less than blue and green. (b) There is also a qualitative effect. The tone of certain colors is changed by the action of the achromatic excitation. The change is greatest when the stimuli are blue and yellow.<sup>22</sup> Yellow, when mixed with black, gives a sensation of olive-green; and blue when mixed with white, black, or gray gives a sensation of reddish-blue.

As a factor influencing the limits and limens of the sensitivity of the retina to color, the inhibitive, or quantitative effect of the fusion concerns us more than the qualitative. As we have stated, a white surrounding field, for example, a white campimeter screen, induces black across the stimulus which fuses with and modifies the resulting sensation; while a black screen induces white. For an estimate of the amount of brightness contrast that is induced by white and black screens across yellow, green, red, and blue stimuli, the reader is referred to the section: *Quantitative Estimate of the Influence of the Change of Illumination upon the Induction of Brightness by the Surrounding Field* (p. 138). The question is considered in detail in that section rather than in the present one, because it will be necessary at that point to compare the amounts of brightness induced by the white and

<sup>21</sup> Ferree and Rand. An Experimental Study of the Fusion of Colored and Colorless Light Sensation: The Locus of the Action. Journ. of Philos. Psychol. and Scientific Methods, 1911, VIII., pp. 294-297. This is only a brief preliminary report of the work. A full report will be published later.

<sup>22</sup> How far the qualitative effects of the fusion of colored with colorless light sensation in central vision are paralleled in peripheral vision, will form the discussion of a later chapter of this investigation, not reported in this paper.

black screens at standard and decreased illumination. In that section is shown also in what way the amounts of brightness induced by the screens were estimated, and within what limits the values obtained can be said to represent these amounts. Tables XII and XIII (pp. 142-143), columns 1, 2 and 3, give the amount of contrast that is induced by the white and black screens at standard illumination across the grays of the brightness of the colored stimuli at  $25^\circ$  and  $40^\circ$  in the horizontal temporal meridian for Observers *A* and *C*. The results of these tables may be summarized as follows:

1. The amount of induction from the white and black screens increases with the distance from the fovea.

2. The amount of induction from the white screen is greater than that from the black screen.<sup>23</sup>

3. The white and black screens induce most across the stimuli that are farthest removed from them in brightness, and least across those which are nearest to them in brightness. That is, the white screen induces more black across the gray of the brightness of blue than across the gray of the brightness of yellow; the black screen induces more white across the gray of the brightness of yellow than across the gray of the brightness of blue.

The effect of this induction of the surrounding field may be shown by two methods: (*a*) by its effect on the limits of color sensitivity; and (*b*) by its effect upon the limens of color sensitivity.<sup>23a</sup> Up to this time, so far as the writer knows, the effect of the surrounding field has been estimated only by the first of these two methods, by its effect on the color limits. This method, however, estimates the effect of the surrounding field upon the color sensitivity of the extreme peripheral retina alone. By the

<sup>23</sup> See footnote p. 141.

<sup>23a</sup> Since sensitivity to color is measured by determining both the limen and j. n. d. of color, it might be thought that the effect of surrounding field could be measured in both of these ways. The determination of the j. n. d. would, however, show very little, because the induction of the surrounding field would affect both the standard and comparison surfaces. This will be true also of the effect of the brightness of the preexposure, and of changes in the general illumination. In none of these cases has the writer considered it worth while to make the determination of the j. n. d.

second method, on the other hand, this effect can be measured in the central and paracentral regions, as well as in all parts of the peripheral retina. In order to make a complete study of the effect of the brightness of the surrounding field on color sensitivity, we have used both of these methods. The report of the work done by them is as follows:

*a. The effect of the induction of the surrounding field upon the limit of color sensitivity.*

Assuming that the law of brightness inhibition of color for the central retina holds for the peripheral retina, we should expect to find that, since colors have a lower limen in black than in gray or white, a white screen, which causes black induction across the stimulus, would be more advantageous to color vision than would a black screen, which causes white induction. Further, we should expect to find that a gray screen of the brightness of the stimulus, which causes no induction whatever, would be the most favorable.

An investigation of the color limits with screens of white, black, and gray of the brightness of the color, shows, however, the following facts:

1. Blue and green have widest limits with the gray screen, slightly narrower with the white, and narrowest with the black.
2. Red and yellow have widest limits with the black screen, slightly narrower with the gray, and narrowest with the white.

The color limits of Observers *A* and *C*, taken on the temporal and on the nasal meridian, are given in Table V and VI.

*b. Explanation of the effect of the induction of the surrounding field on the limits of color sensitivity.*

Turning to the explanation of these results, we shall here endeavor to account for the results obtained with the white and black screens. We have the following points to explain: (*a*) Blue and green have wider limits with the white screen than with the black, but the difference is comparatively small. According to the law of the action of white and black on colors, formulated from the results of work in the central retina, we should expect to find wider limits with the white screen, which induces black, than with the black screen, which induces white. Thus far, then, the results are in accord with the law, but the difference found

TABLE V.

A. Showing the limits of color sensitivity with screens of white, black and gray of the brightness of the color.

Stimulus	Limit with gray screen of the brightness of the color	Limit with white screen	Limit with black screen	Meridian
Yellow	44°	42°	45°	90° Temporal
Green	37°	36°	34°	
Red	43°	42°	44°	
Blue	53°	50°	49°	
Yellow	90°	88°	92°	90° Nasal
Green	64°	62°	60°	
Red	89°	87°	89°	
Blue	92°	92°	92°	

TABLE VI.

Observer C.

Yellow	49°	46°	50°	90° Temporal
Green	44°	42°	40°	
Red	45°	41°	45°	
Blue	56°	55°	53°	
Yellow	92°	92°	92°	90° Nasal
Green	87°	84°	53° <sup>24</sup>	
Red	92°	92°	92°	
Blue	92°	92°	92°	

between the inhibitive action of white and of black in the central retina would lead us to expect a greater effect on the limits. (b) Yellow and red have wider limits with the black screen than with the white. This is in direct contradiction to the law of fusion formulated for the central retina. With regard to explanation, two points must be considered. (1) The relative inhibitive action of black and white upon color must be investigated in peripheral vision; and (2) the rate of falling off in sensitivity of the peripheral retina must be ascertained.

<sup>24</sup> In this case, the qualitative change of green to blue caused the decided narrowing of the limit.

(1) *The relative inhibitive action of black and white upon color in peripheral vision.* The relative inhibitive action of white and black upon the colors must be investigated at all points from the fovea to the limits of sensitivity to see whether the law established for the central retina holds for all degrees of excentricity. If we find that the difference between their inhibitive actions lessens as we go towards the limits, we have a reason for the small widening of the zones of blue and green by the white screen. And if we find just within the limits of sensitivity for red and yellow that black inhibits these colors more than white does, we have a reason for the relative widening of the zones of sensitivity for these colors with the white screens, provided we can show that the effect of neither screen will carry the limits farther towards the fovea than the inner margin of this zone within which the exception is found.

To test the relative inhibitive power of black and white in the peripheral retina, the limen of color in black and white had to be determined. Two methods of procedure were possible with the apparatus used. By the first method, the stimulus was a disc with sectors of color and white or black which could be adjusted so that a liminal sensation of color was produced. In order to prevent brightness induction the screen had to be of a gray of the brightness of the stimulus used. With each addition of color to the stimulus, a change of the brightness was produced. The screen then had to be altered in brightness by an equal amount. Of the two methods of determining the brightness of the stimulus, described p. 91, the method of comparing the brightness of the colorless peripheral sensation with the surrounding field was obviously better adapted to the requirements of this observation than was the more cumbersome flicker method because the brightness of the stimulus was being continually altered. For the present case, the gray squares were used for surrounding field that had served a similar purpose for the determinations of the brightness of the stimuli in the periphery at standard and decreased illumination. The observer first made a preliminary judgment of just noticeable color, and then determined the gray that was equal in brightness to the stimulus. A square of this gray was then

mounted on the campimeter and the final determination of the limen was made. By the second method, the screens were removed and the skeleton apparatus alone was used. A disc composed of white and black sectors was placed on the motor so that it just filled the large circular ring at the center. This gave a surrounding field whose brightness could be adjusted at will. A small disc, 2 cm. in diameter, composed of sectors of the color to be investigated and black or white, was placed over the large black and white disc. The method of procedure was as follows. The observer took the required fixation, and observed the small disc to find the smallest amount of color that could be sensed when fused with white or with black, as the case happened to be. Before each determination, the experimenter adjusted the black and white sectors of the large disc, so that they equalled the brightness of the inner disc. This brightness was readily calculated from the following quantities:—the number of degrees in the colored sector, its black-white-value, and the number of degrees of white or black in the remainder of the disc.

Since the point in question was of considerable importance, both of these methods were used, the one as a check on the other. The first had the advantage of greater ease of manipulation and of employing a stimulus which was the same size as that used in the sensitivity experiments. The second had a possible advantage in the adjustment of the brightness of the surrounding field, but it was of disadvantage because the surrounding field could not be made so wide as by the former method and because a stimulus larger than that usually employed had to be used.

Results from both of these methods show the following facts: (1) As the fixation becomes more excentric, the difference in the inhibitive action of white and black decreases. (2) From center to periphery, the limens of green and blue are greater when mixed with white than when mixed with black; that is, the law of the greater inhibitive power of white holds for these colors in the periphery as well as in the center. (3) An exception to this law is found for yellow and red near the limits of sensitivity. From the center to within about  $5^{\circ}$  of the limit of sensitivity,<sup>25</sup> white

<sup>25</sup> By the limit of sensitivity is meant the widest limit of color determined at standard illumination.

has a greater inhibitive power than black over these two colors. But from this point to the limit, the reverse relation obtains, and red and yellow in this region have a greater limen in black than in white. How much this apparent exception to the law of fusion as it obtains in central vision is due to the natural darkening of red and yellow as they pass into the peripheral field of vision, we are not at this time prepared to state. Because of this darkening, there is more black fused with red and yellow than the results of Table VII express. These results represent the values of the colored and black sectors in the stimulus discs only and not the actual proportions of color and black excitations aroused.

Results are shown in detail in Table VII. They are taken from the records of Observer *A*, on the temporal meridian by the first method described. Column 1 indicates the stimulus used; column 2, the fixation at which the liminal determination was made, and columns 3 and 4, the limens of color mixed with white and black.

TABLE VII.

*A. Showing the inhibitive action of white and black upon color in peripheral vision.*

Stimulus	Fixation	Limen of color in white	Limen of color in black
Yellow	0°	40°	3°
	35°	85°	65°
	38°	95°	85°
	40°	120°	115°
	42°	290°	320°
Green	0°	45°	5°
	25°	80°	50°
	31°	130°	100°
	33°	200°	175°
Red	0°	30°	3°
	35°	80°	65°
	38°	120°	110°
	39°	135°	135°
	40°	155°	170°
	42°	290°	310°
Blue	0°	60°	10°
	35°	125°	65°
	41°	145°	140°
	42°	180°	170°
	51°	300°	280°

(2) *The rate of falling off in the sensitivity of the retina to*

*color from center to periphery.* To determine the falling off in sensitivity of the retina, the limen of color must be known at several points of excentricity. For this determination, the results given in Table VIII, which shows the limens of color when the brightness influence of the screen has been eliminated, best serve our purpose. They show that at  $5^\circ$  from the limit, the limen has been increased from three to tenfold as compared with the limen at  $25^\circ$ , or six to fourteenfold as compared with the limen at the center. The distance between the point  $25^\circ$  from the center, and the point  $5^\circ$  inwards from the limit averages for all colors about  $10^\circ$ . It is readily seen that the sensitivity falls off much faster from the point  $25^\circ$  from the center to  $5^\circ$  from the limit than it does from the center to the  $25^\circ$  point. At the point  $3^\circ$  inwards from the limit, the limen ranges from  $145^\circ$  of color, in the case of blue and green, to  $150^\circ$  of color, in the case of yellow and red. It is from this point that the sensitivity falls off with extreme rapidity. As was mentioned earlier in the discussion (see p. 105), a change in the fixation of  $1^\circ$  peripheralwards causes an increase in the limen of  $65^\circ$  or more, an increase that represents a greater lessening of sensitivity in  $1^\circ$  of excentricity than there was in the first  $25^\circ$  from the fovea.

Values of the limen for all colors with gray screens of the brightness of the color at  $0^\circ$  and  $25^\circ$  from the center, and  $5^\circ$ ,  $3^\circ$ ,  $2^\circ$ , and  $1^\circ$  from the limit are shown in Table VIII. They were determined in the temporal meridian of Observer A and are selected as typical. An equal zone of rapidly decreasing sensitivity was found on the nasal meridian also in every case where the limit of color sensitivity occurred within the range of our apparatus.

In Tables V and VI, it was shown that the limits of color are not changed more than  $5^\circ$  with the white and black screens from their values with screens of the brightness of the color used. The results of Table VIII show why this is so. The comparatively large amounts of induction by the white and the black screens narrow the limits so little because of the extreme rapidity with which sensitivity falls off in this zone. To narrow the limits even  $3^\circ$ , enough brightness must be induced, roughly speaking, to completely inhibit more than  $200^\circ$  of color.

TABLE VIII.

A. *Showing the rapid falling off in sensitivity of the extreme peripheral retina.*

Stimulus	Limen at 0°	Limen at 25°	Limen 5° from limit	Limen 3° from limit	Limen 2° from limit	Limen 1° from limit	Limit
Yellow	18°	35°	100°	150°	240°	330°	44°
Green	20°	40°	130°	145°	260°	345°	37°
Red	9°	17°	132°	150°	200°	320°	43°
Blue	9°	12°	130°	145°	200°	310°	53°

The following points, then, needed in our explanation of the influence of the white and black screens on the limits of color sensitivity have been established. (a) The white screen, which induces black, narrows the limits of sensitivity to red and yellow more than the black screen, which induces white, because neither screen narrows the limit more than 5°, and within this zone of 5°, red and yellow are inhibited by black more than by white. (b) The limits of blue and green are narrowed by the black screen more than by the white screen, because within this zone of 5°, as at the center, these colors are inhibited more by white than by black. But they are narrowed less by the black screen than might be expected from the inhibitive action of white found to obtain at the center, because as we go towards the periphery, the difference between the inhibitive actions of white and black decreases. And (c) neither screen narrows the limits for any color more than 5°, because within the zone 5° from the limits, the sensitivity falls off so abruptly from point to point that more brightness action is required to change the limits beyond this amount than either the white or the black screen induces.

We have explained the limits of sensitivity to the four colors when black and white screens are used. We have still to explain the results obtained with the gray screen. Since it causes no brightness induction, we might expect our widest limits to occur with this screen. Table V and VI, however, show that while this is true to some extent for blue and green, it is not true for red and yellow. The limits for red and yellow with the gray screen

of the brightness of the color are in each case slightly narrower than with the black screen and wider than with the white. As we are still working on this point, we do not at present feel justified in saying anything final by way of explanation. We may point out, though, that red and yellow darken in passing into the peripheral field of vision. The black screen tends to lessen this effect by contrast, and the white screen to augment it. It seems reasonable to expect, then, that the black screen, which lessens, by means of the white contrast, the amount of black fused with these colors in darkening, would widen their limits; and that the white screen, which increases it by means of black contrast, would narrow their limits, as compared with the gray screen, which exerts no effect at all. We can speak only tentatively, however, until the amounts of brightness dealt with in each case can be more accurately ascertained.

*C. The Effect of the Induction of the Surrounding Field upon the Color Limens.*

In order to estimate the effect of the induction of the surrounding field upon the limen of sensitivity to the different colors, the limens of color were determined at the center, and at  $15^\circ$ ,  $25^\circ$ , and  $30^\circ$  of excentricity in the peripheral retina (*a*) when the surrounding field was of the gray of the brightness of the color; (*b*) when it was white; and (*c*) when it was black.

The preëxposure was in each case to the gray of the brightness of color. The limen was determined as follows: The stimulus composed of sectors of the color and the gray of the brightness of the color at the excentricity for which the limen was to be determined, was placed on the motor behind the campimeter screen. The proportions of the sectors were changed until the observer made the judgment of just noticeable color. Judgments were taken in ascending and descending series, and the average was taken as the value of the limen.

The results show that the influence of the brightness of the surrounding field upon the color limen is as follows:

1. The limen is lowest when the surrounding field is of the gray of the brightness of the color.

2. The difference in the effect of the white and black screens upon the limen increases from the fovea outwards.

3. For yellow and green the limen is highest when the field is black and the induction white, and lower when the field is white and the induction black.

4. For red and blue, the limen is highest when the field is white and the induction black, and lower when the field is black and the induction white.

5. The difference in the effect of white and black screens on the limens is not so great as one at first thought might be led to expect from the results obtained by the objective mixing of white and black with color in the central retina.

The results for Observer *A* are given in detail in Table IX.

D. *Explanation of the Effect of the Induction of the Surrounding Field upon the Color Limens.*

We have, then, the following facts to explain: (1) The limen of sensitivity to color is lowest when the surrounding field is of the gray of the brightness of the color. This is what should be expected, because in case of this screen there is no induction present to fuse with the color sensation, and to affect the limen of sensitivity. (2) The difference in the effect of the white and black screens increases from the fovea outwards. This is because the sensitivity of the retina to brightness contrast increases from the fovea outwards, as the table for the amounts of induction shows. More white and black, then, are induced, and as our results with objective mixing show, the greater are the amounts of white and black mixed with color, the greater is the difference between the inhibitive actions of equal amounts of each.<sup>26</sup> (3) The limen of sensitivity to yellow and green is high-

<sup>26</sup> A rough demonstration of this can be easily made as follows. Set up two discs, of blue for example, side by side on color-mixers. Add a small sector of white to the one and an equal sector of black to the other, and observe the apparent saturations of each. Repeat the observation several times, each time increasing the sectors of black and white by equal amounts. It will be observed that the difference in the apparent saturations of the equally saturated discs becomes greater and greater, until at 180° the disc to which white was added appears almost colorless while the disc to which black was added is still a well-saturated dark blue.

est when the surrounding field is black, and lower when the surrounding field is white. This is in accord with the general law of the inhibitive action of white and black on color. That is, since color is inhibited less by black than by white, we should expect in terms of the law that the limen of color would be lower with the white screen which induces black than with the black screen which induces white. The limens obtained for yellow and green present no exception to this law. (4) The limens for red and blue are highest when the surrounding field is white, and lower when the surrounding field is black. But this is in apparent contradiction to our general law of the relative inhibitive action of white and black upon the colors. An explanation of why we have this apparent contradiction in case of red and blue and not in case of yellow and green may be readily found, however, in the relative amounts of contrast induced by the white and black screens across these colors. Table XII (p. 142) shows the amount of contrast that is induced by the white and the black screens across the grays of the brightness of the colors. As we have already mentioned, the white screen induces more black across the grays of the brightness of red and blue, than of yellow and green; the black screen induces more white across the grays of the brightness of yellow and green, than of red and blue. For example, Observer *A* estimated the amount of black induced by the white screen at  $25^{\circ}$  in the horizontal temporal meridian as  $135^{\circ}$  for yellow, and  $155^{\circ}$  for green; and the amount of white induced by the black screen as  $110^{\circ}$  for yellow, and  $60^{\circ}$  for green. There is, then, less white induced across these two colors by the black screen than there is black induced by the white screen. In spite of this, however, the greater inhibitive power of this smaller amount of white is sufficient to raise the limen of sensitivity to yellow and green slightly higher than it is raised by the less inhibitive power of the larger amount of black. For red and blue, on the other hand, the black induced by the white screen is estimated as  $230^{\circ}$  for red, and  $290^{\circ}$  for blue; while the white induced by the black screen is estimated as only  $28^{\circ}$  for red and only  $12^{\circ}$  for blue. In these cases there is a very much greater amount of black induced than of white.

And this very much greater amount of black is sufficient to raise the limen of sensitivity to the colors with which it is fused higher than it is raised by the very small amount of white, in spite of the fact that when equal amounts of black and white are mixed with a color, its saturation is inhibited much more by white than by black. (5) The difference in the effect of white and black screens on the limens is not so great as one at first thought might be led to expect from the results obtained by the objective mixing of white and black with color in the central retina. This may be explained as follows. (1) The relative amounts of white and black induced upon the different colors by the screens vary greatly. We have thus not a simple case of a difference in the inhibitive action of equal amounts of black and white. In case of yellow and green, for example, there is so much more black induced than white that the white raises the limen very little more than the black. And in case of red and blue, the amount of black induced is so very much in excess of the white that the limen is raised even more by the black than by the white. It is not raised much more, however, (even less than the excess for white in case of yellow and green), because (a) the excess of black induction is not sufficiently large greatly to overweigh the superior inhibitive power of white; and (b) the difference between the inhibitive powers of white and black is high for red and blue, especially for blue. (2) The difference in the inhibitive power of white and black on colors decreases from the center to the periphery of the retina. Thus not so great a difference is found in the limens for white and black screens in the peripheral retina as one might be led to expect from the amounts of induction present. An inspection of the table shows that the difference in the limens for the white and black screens increases from the center towards the periphery, but this increase caused by the greatly increased amounts of induction<sup>27</sup> is not so great as it would have been, were there no decrease in the difference in the inhibitive power of white and black on the different colors.

<sup>27</sup> It has already been shown, footnote, p. 121, that the greater are the equal amounts of white and black added to color, the greater will be the difference in the inhibitive actions exerted by these equal amounts.

TABLE IX

A. *Showing the limens of color sensitivity with screens of white, black, and gray of the brightness of color.*

Stimulus	Point on horizontal temporal meridian at which limen was taken	Limen with screen of gray of brightness of color	Limen with white screen	Limen with black screen
Yellow	0°	18°	22°	28°
	15°	22°	25°	35°
	25°	35°	50°	65°
	30°	50°	80°	95°
Green	0°	20°	22°	28°
	15°	27°	30°	35°
	25°	40°	50°	75°
Red	0°	9°	13°	10°
	15°	9°	19°	15°
	25°	17°	30°	23°
	30°	25°	50°	29°
Blue	0°	9°	17°	10°
	15°	10°	25°	12°
	25°	12°	35°	18°
	30°	20°	40°	30°

We have explained the effect of the induction of the surrounding field on the limits of color sensitivity, and on the limens of sensitivity. As we have said, the effect on the limit takes place in the extreme peripheral retina; the effect on the limen has been measured in the more central regions of the retina,—at 0°, 15°, 25°, and 30° of excentricity. We have remaining to compare the effect of the induction of the surrounding field in the extreme peripheral retina, as estimated by the limit, with its effect in the more central regions, as estimated by the limen, and in turn to determine how both sets of effects harmonize with our law of the inhibitive action of brightness on the colors. The comparison of the results obtained by the two methods for each of the colors is as follows:

1. For yellow, the limen of sensitivity was lower with the white screen but its limit was wider with the black screen. The effect of the screen upon the limen for this color is in accord with our general law of the relative inhibitive action of white and black upon the colors; and the effect of the screen on the limit

is in accord with its exception formulated for the extreme peripheral retina; that is, that in the region  $5^\circ$  from the limit for yellow, black inhibits yellow more than white does.

2. For green, the limen was lower and the limit was wider with the white than with the black screen. The effect of the induction of the screens, then, on both limens and limits, is in accord with our general law.

3. For red, the limen was lower and the limit was wider with the black than with the white screen. The effect of the induction of the screens on the limen is not in accord with our general law that white inhibits color more than black; however, the exception is readily explained by the much greater amount of black than of white that is fused with the color sensation by the induction of the screens. The effect on the limit is in accord with our exception to this law formulated for the extreme peripheral retina; that is, that within the region  $5^\circ$  from the limit of sensitivity to red, black inhibits red more than white does.

4. For blue, the limen was lower with the black screen, but the limit was wider with the white screen. The effect of the induction of the screen on the limen is not in accord with our general law, but the exception may be explained, as in case of red, in terms of the very much greater amount of black induced by the white screen than of white induced by the black screen. We have here, however, an apparent paradox with regard to the limits. That is, since the law of the relative inhibitive action of white and black is the same at the limit for blue as it is at the center, we might expect that if the black induction was sufficiently in excess of the white to make the limens higher for the white screen than they were for the black, it would also correspondingly make the limits narrower for the white screen than for the black. The reverse, however, it will be remembered, was true. The reason for this lies in the fact often mentioned previously that blue lightens in the periphery, so that near its limit of sensitivity it is not in so much greater contrast to the white screen than to the black screen as it is in the center. For example, for Observer *A* the brightness of blue in the periphery

equalled gray No. 28. In the periphery, then, the amount of white induced by the black screen is sufficient to inhibit the blue sensation more than it is inhibited by the amount of black induced by the white screen. It may be mentioned, however, that the difference between the limits for blue with the white screen and with the black screen is smaller for all observers used than is the difference between the limits with these screens for any other color with which we worked (see Tables V and VI, p. 114).

### 3. *The Brightness of the Preëxposure.*

When making the color observation in the peripheral retina, the observer is given a short period of preparation before the stimulus is exposed, in which to obtain and hold a steady and accurate fixation. This introduces the factor of preëxposure, for during this period of preparation, the area which is to be stimulated by color receives a previous stimulation. It seems strange to the writer that this factor, which exerts a greater influence over the extent of color sensitivity than any we are examining, with the possible exception of large changes in the general illumination, should have been so generally overlooked in the work of earlier investigators. It has always been considered a sufficient precaution to eliminate all color from the preëxposure. This, however, is not enough. It should also be of the same brightness as the color by which the eye is to be stimulated. If not, it gives an after-image which mixes with the succeeding color sensation and both reduces its saturation and modifies its color tone.<sup>28</sup> If the preëxposure is lighter than the stimulus color, it adds by after-image a certain amount of black to the succeeding color impression; if darker, it adds a certain amount of white. Since white inhibits color more than black, the effect of a dark preëxposure is to reduce the sensitivity to color more

<sup>28</sup> This action takes place apparently at some physiological level posterior to the seat of the positive, negative, and contrast color processes commonly supposed to be located in the retina. (See Ferree and Rand. An Experimental Study of the Fusion of Colored and Colorless Light Sensation: The Locus of the Action. Journ. of Philos. Psychol. and Scientific Methods, 1911, VIII., pp. 294-297.)

than the effect of a light preëxposure.<sup>29</sup> But since both white and black as after-effect reduce the sensitivity to color, the eye is rendered more sensitive when no after-image is given, that is, when the preëxposure is of the same brightness as the color. The preëxposure should, therefore, be to a gray of the brightness of the color. No brightness after-image will be added to the succeeding color impression to modify either its saturation or its color tone. Even closing the eye, as is frequently done before stimulating, is equivalent to giving a black preëxposure.

No thought apparently was given by previous experimenters to the intense after-effect which follows the exposure of the eye to a brightness quality differing from that of the stimulus. Hess,<sup>30</sup> Fernald,<sup>31</sup> and Thompson and Gordon,<sup>32</sup> it is true, covered the stimulus before exposure with a card matching in quality the campimeter screen, but since the campimeter screen was not always of the same brightness as the color used for the stimulus, this by no means ruled out the effect of preëxposure. The motive of each of these experimenters seems to have been to standardize the observation for the effect of preëxposure, but no notion of its action sufficiently clear to guide them in formulating their technique seems to have been entertained. Since the action of the preëxposure is by way of arousing a brightness after-image, it is obvious that the preëxposure card should, as stated above, be matched in brightness to the stimulus color rather than to the screen.

In the articles, "*Colored After-Image and Contrast Sensa-*

<sup>29</sup> A very striking demonstration of the effect of preëxposure upon the sensitivity of the retina to color can be made for class or lecture room purposes as follows. Mount a sheet of the blue paper of the Hering series on cardboard. Cover one-half of another sheet of cardboard of the same size with white of the Hering series of papers, the other half with velvet black. Place this card immediately in front of the first card and fixate its center for 10 or 15 seconds. Remove and observe the comparative effect of the white and black preëxposures thus obtained upon the color impression gotten from the blue surface.

<sup>30</sup> Hess, C. loc. cit.

<sup>31</sup> Fernald, G. M. Psychol. Rev., 1905, XII., p. 394; Psychol. Rev. Monog. Sup., 1909, X., No. 42, p. 17.

<sup>32</sup> Thompson and Gordon. A Study of After-images on the Peripheral Retina. Psychol. Rev., 1907, XIV., p. 123.

tions from Stimuli in Which No Color Is Sensed,"<sup>33</sup> and "*The Fusion of Colored with Colorless Light Sensation.—The Physiological Level at Which the Action Takes Place*,"<sup>34</sup> the effect of the after-image due to previous brightness exposure upon color sensitivity has already been shown for both central and peripheral retina. The general fact need not further be dwelt on here. We do, however, need to show why in the peripheral retina the short preëxposure which takes place while the eye is obtaining a steady fixation has so much effect upon the color stimulation immediately following. Two reasons are found for this. (a) The peripheral retina is extremely sensitive to short stimulation. While some slight variation is found at different angles of excentricity, the peripheral after-image reaches in general its maximal intensity with two or three seconds stimulation. This amount of time is usually consumed in obtaining fixation, hence in each observation there is fused with the color sensation about as strong a brightness after-image as can be aroused. For this reason alone, it is readily seen why the brightness of the pre-exposure is of so much greater consequence in the peripheral retina than it is in the central retina, where the maximal strength of the after-image is obtained with from forty to sixty seconds stimulation. (b) There is apparently no latent period in case of the peripheral after-image. It flashes out at full intensity immediately upon the cessation of the stimulus. Thus, there is no possibility of escaping the full effect of the brightness after-image upon the stimulus color, as might happen in the central retina, where the latent period obtains, if there were a very short exposure to the stimulus color.

If when working with the campimeter, for example, a black card is used to cover the stimulus-opening during the period of preparation, an intensive white after-image is aroused which

<sup>33</sup> Ferree and Rand. Psychol. Rev., 1912, XIX., pp. 195-239.

<sup>34</sup> For abstract of the article, see Journ. of Philos. Psychol. and Scientific Methods, 1911, VIII., pp. 294-297. The article will soon be published in full. See also Ferree, C. E. Tests for the Efficiency of the Eye Under Different Systems of Illumination and a Preliminary Study of the Causes of Discomfort. Transactions of the Illuminating Engineering Society, 1913, VIII., pp. 40-60.

fuses with the succeeding color sensation, strongly reducing its saturation. If, on the other hand, a white card is used, a black after-image is obtained, which, according to our law of the action of the achromatic sensation upon color, has less effect than the white after-image upon blue and green, and also upon red and yellow if the after-image is sufficiently strong to narrow the limits to red and yellow more than  $5^{\circ}$ . In each case, the intensity of the after-image will in part depend on the brightness of the subsequent color exposure, the projection field. The after-image due to preexposure to white will be more intensive when blue than when yellow forms its projection field. The after-image from black will be more intensive when projected on yellow or green than on blue or red. If, however, a gray card of the brightness of the stimulus color be used as preexposure, there will be no after-image to modify the color sensation. The only brightness change acting upon it will be due to the slight adaptation to this gray during the short time of preexposure.

The method, then, of eliminating the effect of preexposure consists in making it of the brightness of the color to be used as stimulus. And in case the brightness of the color alters in passing from the center to the periphery of the retina, the brightness of the preexposure must be correspondingly altered. For example, at standard illumination, Hering Gray No. 41, or its equivalent should be used for preexposure to blue when the central retina is investigated. But for the peripheral retina, a much lighter gray should be used because in this region blue lightens by an amount depending upon the excentricity of the stimulation and in part upon individual variation.

#### *A. Effect upon the Limens of Color and upon the Limits of Color Sensitivity.*

To test the importance of preexposure, two methods of measurement were employed. In the first, the limen of color was obtained at  $0^{\circ}$  and at  $35^{\circ}$  on the temporal meridian, when the screen was of the gray of the brightness of color, and the preexposure was in turn to the same gray, to white, and to black. In the second method, the limits of color sensitivity were investigated under the same conditions of campimeter

screen and preexposure card. The results for Observer *A* are recorded in Table X and in rows 1, 4, 7, 10 of Table XI. They show in every case, (a) that the limen is raised and the limit is considerably narrowed when the preexposure is not to the gray of the brightness of the color, that is, when it gives a brightness after-image; and (b) that the limen is higher and the limit narrower when the preexposure is to black and its after-image is white, than when the preexposure is to white and its after-image black.<sup>35</sup> This is in accord with the law of the

<sup>35</sup> There is one exception to this statement. The limen for blue at  $0^\circ$  for both white and black preexposures is  $13^\circ$ . The following reasons may be given for this. (a) It is one of the fundamental laws of brightness after-images that the intensity of the after-image depends in part upon the brightness relation of stimulus to projection field. When the brightness difference between the stimulus and the projection field is small, a weak after-effect is obtained; when it is greater, a more intensive after-effect is obtained. Blue, as aroused in the central retina, is very near to black in brightness, and very far removed from white. We should then expect a very much more intensive after-effect of the exposure to white than to black when the projection field is blue. (b) The brightness relation of the surrounding field to the preexposure also exerts an effect on the intensity of the after-image given by the preexposure. When the surrounding field differs in brightness from the preexposure, contrast is induced. This contrast quality in turn also gives an after-image which mixes with and modifies the after-image given by the preexposure. When the surrounding field is of the gray of the brightness of blue, for example, and the preexposures are in turn white and black, the influence of the surrounding field is to make the after-image of white stronger than that of black. That is, when the preexposure is white, this white is strongly intensified by contrast with the surrounding field of dark gray, and in consequence the black after-image is strongly intensified. But when the preexposure is black, little intensification results by contrast with the surrounding field and little effect is had on the after-image. Both of these influences, then, tend to cause much more black to be added to a blue stimulus in the central retina as a result of preexposure to white than white to be added as a result of preexposure to black. The effect of this excess of black, as is shown by the table, is to raise the limen for blue for the white preexposure as much as it is raised by the black preexposure; in other words, to make the limens equal. At  $35^\circ$  in the periphery, however, the limen of blue is seen in the table to be higher when the preexposure is to black and the after-image white, than when the preexposure is to white and the after-image black. We have here a different case. The difference in the effect of white and black preexposures at  $0^\circ$  and at  $35^\circ$  is due to the fact that at  $0^\circ$  blue is very dark while at  $35^\circ$  in the periphery it has lightened until its brightness equals No. 35 gray. In the latter case (a) there is less difference than there was at  $0^\circ$  between the brightness relations of the stimulus to

action of brightness upon color and holds for all colors used. It might be expected from the exception to this law, mentioned on page 116, that the limits for red and yellow would be narrowed more by the black after-image than by the white. This is not found to be true because the effect of each after-image is sufficiently strong to narrow the limits of red and yellow more than  $5^\circ$ , and thus to carry the limits for these colors outside of the zone in which they are inhibited more by black than by white.

*B. Combined Effect of Surrounding Field and Preëxposure upon the Limits of Color Sensitivity.*

The effect of preëxposure upon color limits was also investigated when white and black screens were used. The results obtained are of interest in two regards. (a) They show under these typical conditions in what way and to what extent the inductive action of the screen combines with the effect of preëxposure to modify the limits of color. (b) They help to explain some of the conflicting results obtained by previous investigators who did not carefully standardize their observations with reference to the effect of preëxposure and surrounding field. A few points may be noted in advance of the tables showing the combined action of preëxposure and surrounding field upon the extent of color sensitivity. It is obvious that when the campimeter screen is either white or black and the preëxposure is to the same brightness quality, there will be no inductive

the white preëxposure and of the stimulus to the black preëxposure, and consequently less difference between the intensities of the after-images from these preëxposures; and (b) there is less difference than there was at  $0^\circ$  between the brightness relations of the surrounding field, which is of the brightness of blue at the point at which we are working, to the white preëxposure and of the surrounding field to the black preëxposure, and for this reason also, less difference between the intensities of the after-effect of the contrast induced by the surrounding field upon these preëxposures. For both reasons, therefore, the after-image from the white preëxposure at  $35^\circ$ , when projected on blue, is not so much more intense than the after-image from the black preëxposure, as it was at  $0^\circ$ . At  $35^\circ$ , then, in these experiments the white after-effect due to preëxposure to black is sufficiently intensive to raise the limen of blue higher by virtue of its greater inhibitive power than it is raised by the black after-image due to preëxposure to white.

action by the screen upon the preëxposure either to intensify or to weaken it. In this case both preëxposure and screen will add the same brightness quality to the stimulus color, the former by contrast, and the latter by after-image. The effect of this action upon the color limits is shown in Table XI, Column 4, rows 2, 5, 8, 11; and column 5, rows 3, 6, 9, 12. If, however, the preëxposure and the screen are different in quality, their action may be either antagonistic or supplementary, depending upon the brightness relations between the screen and the preëxposure, on the one hand, and between the screen and the color sensation fused with the after-image of preëxposure on the other. For example, if a black preëxposure and a white campimeter screen are used, the white screen will intensify the blackness of the preëxposure by contrast but will tend to darken the fusion of color sensation and white after-image, and thus will lessen the action of the latter upon the color. This effect is shown in Table XI, in column 4, rows 3, 6, 9, 12; and column 5, rows 2, 5, 8, 11. If, however, the preëxposure is black, and the campimeter screen is the gray of the stimulus color, the

TABLE X.

A. *Showing the effect upon the color limens of preëxposure of the gray of the brightness of the color used, of white, and of black.*

Stimulus	Surrounding field	Preëxposure	Limen at 0°	Limen at 35°
Yellow	gray no. 2	gray no. 2	18°	83°
		white	35°	105°
		black	45°	125°
Green	gray no. 8	gray no. 8	20°	100° <sup>38</sup>
		white	33°	155°
		black	40°	180°
Red	gray no. 24	gray no. 24	9°	90°
		black	13°	135°
		white	20°	148°
Blue	gray no. 41 at 0° no. 35 " 35°	gray	9°	30°
		white	13°	40°
		black	13°	43°

<sup>38</sup> As green has a narrow zone of visibility, the limen was taken at 30°.

screen will intensify the blackness of the preëxposure by contrast, but will lighten the fusion of stimulus color and white after-image, thus will add to the action of the latter upon the color. This effect is shown in Table XI, columns 4 and 5, rows 1, 4, 7, 10.

TABLE XI.

*A. Showing the combined effect of campimeter screen and preëxposure upon the limits of color sensitivity.*

Stimulus	Campimeter screen	Limit with gray preëxposure of the brightness of the color	Limit with white preëxposure	Limit with black preëxposure
Yellow	gray no. 2	44°	40°	38°
	white	42°	42°	43°
	black	45°	43°	40°
Green	gray no. 8	37°	35°	31°
	white	36°	30°	25°
	black	34°	36°	25°
Red	gray no. 24	43°	38°	37°
	white	42°	38°	39°
	black	44°	43°	38°
Blue	gray no. 28	53°	42° <sup>37</sup>	42° <sup>37</sup>
	white	50°	42° <sup>37</sup>	48°
	black	49°	51°	42° <sup>37</sup>

Having seen from Tables X and XI the effect of preëxposure and the combined effect of preëxposure and surrounding field upon the limen and the limits of color, we may turn our attention to a third point of interest; namely, the explanation of some of the differences between our results and those obtained by earlier investigators in terms of the different conditions of preëxposure used. As Hess and Fernald alone have stated their conditions of preëxposure, we shall have to limit our discussion to them. Hess wished to find the comparative limits of color sensitivity. He attempted to standardize the intensity and the brightness of the stimuli used, and worked with white, black, and gray campimeter screens. The eye of the observer followed

<sup>37</sup> For the explanation of this decided narrowing of the limit for blue, see footnote, p. 108.

a moving fixation-point while the stimulus was exposed from time to time. In every case, the stimulus was covered by a card of the same brightness and quality as the screen. He turned the gray screen toward or away from the source of light until its brightness was such that the color disappeared in the periphery into a gray of the brightness of the screens. He thus worked with screens of the gray of the brightness of the color, of white, and of black, and with preexposures of the same brightness as the screens. In only one of these cases, namely, the first, has he eliminated the effect of preexposure. He found that for every color, the limit of sensitivity was widest with the gray screen, narrower with the black, and narrowest with the white. His results are in contradiction to ours with regard to the influence of these screens, when the factor of preexposure is eliminated (see p. 113). They are, however, very nearly confirmed by the results we obtained when the preexposure was of the same brightness as the screen (See Table XI). Had our illumination been slightly less, and consequently the induction from the white screen greater, our results would have been very similar to those of Hess.

Fernald, working with a white and a black background and using preexposures to match, obtained results that are similar to those of Hess for all colors except red, and even in this case the exception is only apparent. She says: "All the colors except the reds are perceived at a greater degree of eccentricity with the dark than with the light background. Red is seen as red to about the same degree of eccentricity with the dark as with the light background, but is seen as yellow or orange with the dark background at the same points at which it is seen as colorless with the light background."<sup>38</sup> The latter part of the quotation shows that the exception to Hess in case of red was due rather to a difference in the method of measurement than to a difference in result. She recorded as the limit of color the point at which the sensation took on any trace of a foreign quality. Although the red stimulus appeared as red-yellow in the periphery with the black screen, there was some red in the

<sup>38</sup> Fernald, G. *Psychol. Rev. Monog.*, 1909, X, p. 23.

sensation received from it at a greater angle of excentricity with the dark than with the light screen.

As was said above, the conclusion of Hess and of Fernald that the colors have wider limits with the black than with the white screen, is confirmed by our work at certain illuminations when white and black preexposures are used. But, since we do not obtain similar results when the influence of preexposure is eliminated and the effect of the brightness of the field determined in isolation from this factor, we maintain that their results were due to brightness conditions not connected with the surrounding field, but with the preexposure. Our contention is that many of the conflicting conclusions concerning the effect of background upon color limits have resulted from ignorance of the important factor of preexposure. Comparative color limits can be obtained with any screen only when the preexposure is to the gray of the brightness of the stimulus. This principle was used by Hess in his work with the gray screen, but apparently without a definite purpose and without knowledge of its importance.

#### 4. *The General Illumination of the Retina.*

The effect of change of illumination was forced upon our attention early in the investigation of the factors that influence the color sensitivity of the retina. For example, in preliminary work done by the writer on a well-lighted porch on Long Island, changes in color tone were reported, when certain colors were compared in the central and in the peripheral retina, that are not found at all under the more intensive illumination of our optics-room, when neither of the curtains is drawn; and the peripheral limits of color were narrower by  $5^{\circ}$  to  $12^{\circ}$ . Furthermore, on a dark day, it was found that the limits of stimuli exposed through an opening in a white screen were reduced by about  $4^{\circ}$  as compared with the limits taken on a bright day. The change was less considerable with black and gray screens. The change in color tone was most conspicuous in case of green.<sup>39</sup> On dark days the green stimulus appeared as a pale unsaturated blue before becoming colorless in passing from the center to the periphery of the retina. This zone of blue was from  $7^{\circ}$  to  $23^{\circ}$

<sup>39</sup> The green of the Hering series was used.

wide in different meridians of the retina with both white and black screens, but was wider with the black than with the white screen. On a sunny day, on the other hand, with the white screen green passed into bluish-green, then directly into gray, except in case of the upper regions where it appeared blue throughout a zone of about  $4^\circ$  in width. With the black screen, the blue zone was found only in the upper and temporal regions of the retina. The transition of green to yellow in the periphery that is generally reported in the literature was found in these experiments only when the gray screen was used. Yellow showed a color change that varied in amount with the degree of the general illumination. On a bright day, it appeared reddish-orange with the white screen. On a cloudy day, it was seen in the extreme periphery as a dark saturated red.

Working in our optics-room we found also that results taken on one day could not at all be duplicated on the following day. When the work was carried on under the most favorable conditions without special means of controlling illumination, namely, on bright days only, differences of  $5^\circ$  or more were found when the white screen was used. This necessitated a long series of observations if legitimate averages were to be obtained. Such a procedure is at best a poor makeshift and is besides of great disadvantage in many problems that come up in the work on color sensitivity. Particular instances of this may be found in investigations in which it is required to work in the region lying just within the limits of sensitivity, and in work on the after-images of stimuli in which no color is sensed. In the latter case the experiment requires that the stimulus be exposed just outside the limits of sensitivity determined with a given brightness condition, and that the observer should not be aware of the nature of the stimulus. In order to fulfill these requirements the experimenter must know the limits obtaining with a given brightness condition. It would be impossible to know this when the brightness conditions were subjected to the influence of changing illumination unless re-determinations were made at the beginning of each sitting and even frequently during its course. This would consume a great deal of time and would, besides,

only roughly fulfill the requirements of the problem. A further and still more important example of the disadvantage may be found in the task we had set ourselves, namely, to investigate from point to point the sensitivity of the retina to each of the principal colors for three backgrounds in at least sixteen different meridians. In this work it is obvious that unless a standard illumination were provided, all comparative work would have to be done at one sitting. This is impossible. When time is taken between observations to guard against fatigue, at least three hours is required merely to outline the limits of sensitivity for a given color with one background for only one-half of the retina. Even for this length of time there is no guarantee that the illumination has not altered. Thus at the outset of any extended investigation of color sensitivity, it is evident that without a standard illumination, results will be of little comparative value.

In order better to know our factor and the ways in which it operates, a systematic investigation of the influence of changes of general illumination was carried on in our optics-room, which is especially constructed to secure fine changes in illumination. Rough preliminary experiments showed that the primary effect of decreasing the illumination was an alteration of the amount of contrast induced across the stimulus by the campimeter screen. With the white screen, the increased induction was the most pronounced and was sufficient to cause large changes in the limits and in the color tone of the stimulus. In order to investigate this effect in detail, gradual changes of illumination covering a wide range were made by means of the curtains with which our optics-room is furnished, described on p. 86. Attention was given to the following points. (*a*) A quantitative estimate was made of the influence of change of illumination upon the brightness induction of the campimeter screen. (*b*) The effect of this induction upon the limits of color sensitivity was determined. (*c*) The limens of the colors were measured at different degrees of excentricity at different illuminations. And (*d*) the influence of change of illumination upon the effect of the preëxposure on the limens and limits of color was investigated. The degrees

of illumination chosen for comparison were the standard illumination, the method of obtaining which will be described later, and the decreased illumination mentioned above (see p. 95 and Tables I and II) for which the white-values of the stimuli were determined. Measured in foot-candles by means of the Sharpe-Millar portable photometer, the standard illumination equalled 390 foot-candles, the decreased 1.65 foot-candles.

*A. Quantitative Estimate of the Influence of Change of Illumination upon the Induction of Brightness by the Surrounding Field.*

The purpose of this investigation was to find out how much the induction from white and black screens<sup>40</sup> is affected by a change in the general illumination; and (b) how much induction is gotten at decreased illumination from the gray screen which matches the color in brightness at standard illumination. The induction in this latter case is caused by the change in the brightness relation between color and screens with decrease of illumination.<sup>41</sup> The campimeter screens served as inducing surface, grays of the brightness of the four principal colors of the Hering series both at standard and decreased illumination were used in turn as stimuli, and the amount of induction was estimated upon a measuring-disc, made up of adjustable sectors of the gray of the stimulus and white or black, according to the screen used. The measuring-disc was mounted on a motor (see p. 87 and Fig. I) which could be moved along the graded arm of the campimeter to any position from 20° to 92°. The gray stimulus was exposed through the opening of the screen in the usual manner. Two preliminary precautions were observed. (a) Since

<sup>40</sup> White and black screens are chosen because they represent the extreme cases of the effect of change of illumination.

<sup>41</sup> This latter determination is made to show that it is impossible to standardize the brightness of the surrounding field against the sudden and progressive changes of daylight that occur during the course of a single series of observations. These changes alter the brightness relation between the colored stimulus and the gray used as screen; therefore a match made at the beginning of a series will not hold throughout its course. For the same reason and to an equal degree the brightness relation between preëxposure and colored stimulus changes with change of illumination. It is, therefore, equally impossible to standardize the brightness of the pre-exposure without some means of securing a standard illumination.

the brightness of the gray stimulus plus the induction of the screen was to be estimated by means of the measuring-disc, and since the brightness-value of the stimulus and of the disc changes with the amount of light that falls upon them, it was necessary to make sure before each measurement that the same amount of light fell upon each. This precaution was all the more necessary because the stimulus had to be placed behind the screen and the measuring-disc in front. In a given position of the apparatus, one or the other was apt to be shaded. The determination was made as follows: Measuring-disc, campimeter screen, and gray stimulus were all given the same brightness-value according to determinations made under conditions about which no doubt of the equality of the illumination of each could be entertained. Each was then placed in position for the experiment, and the position of the campimeter as a whole and of its various parts was adjusted until stimulus, screen, and measuring-disc were exactly matched in brightness-value. When an exact match was obtained we were guaranteed that all three were again equally illuminated. This precaution was particularly necessary in the investigation we are discussing in this section. It was carefully observed, however, throughout the entire work. (b) The question arose whether brightness induction comes to its maximal value at once in the peripheral retina. A determination of the intensity curve of the contrast sensation was accordingly made at various points in the peripheral retina. It showed that contrast increases strongly for the first few seconds of stimulation. For this reason it was found to be necessary to make the judgment concerning the amount of induction of the screen, just as long after the induction had commenced as was done in the experiments to determine color sensitivity. In the color experiments an interval has to be allowed before the stimulus is exposed during which the observer obtains a steady fixation. During this interval of preexposure, the eye is being stimulated by the campimeter screen and by the card which covers the stimulus. To prevent the preexposure card from giving a brightness after-image which would fuse with and modify the sensation immediately following, it should be chosen of a gray of the

brightness of the color. In the same way, an interval had to be given in which to secure steady fixation when the amount of brightness induction was being measured. In order, then, to have the judgments made in each case the same length of time after induction had begun it was necessary only to make the intervals of preëxposure of equal duration and to require that the judgments of each kind be made directly at the end of the preëxposure. In the case of the color experiments, the signal for the making of the judgment is the withdrawal of the preëxposure card and the exposure of the stimulus. For the judgments of induction, however, in which case the stimulus was the gray of the brightness of the color, it is obvious that no preëxposure card was needed, for preëxposure and stimulus were required by the conditions of the experiment to be the same. In this case, a word-signal had to be given to indicate the termination of the preëxposure interval and the instant at which the judgment was to be made.

*Results when white and black screens were used.*— Observing these precautions as to the equality of the illumination of stimulus, screen, and measuring-disc, and as to the length of time the induction had had in which to increase before the judgment was made, measurements were taken of the induction by white and black screens across grays of the brightness of the four principal colors at the illumination used. These measurements were made at various points of excentricity on the retina, and for both standard and decreased illuminations. The determination of the equality point between the stimulus and the measuring-disc was made as follows: The size of the white or black sector of the latter was changed until a preliminary judgment of equality was made. Then the j. n. d. on either side of this point was determined both by ascending and by descending series and an average of the results was taken as the value of the induction. Measurements were taken at  $25^{\circ}$  and at  $40^{\circ}$  on the temporal meridian, and at  $55^{\circ}$  and  $70^{\circ}$  on the nasal. The conditions at the nasal  $55^{\circ}$  were very similar to those at  $25^{\circ}$  on the temporal side. The measurements at  $70^{\circ}$  nasal were midway in value between those at  $25^{\circ}$  and at  $40^{\circ}$  on the temporal. The  $40^{\circ}$  point is very near the limits of color sensitivity in this meridian, and the induction here is very great. For one ob-

server, the darker stimuli appeared black at this point, when the white background was used. In such cases, the difference between the induction at standard and at decreased illumination is more clearly shown by the observations made at  $25^\circ$  temporal meridian and at  $55^\circ$  and  $70^\circ$  nasal meridian than at  $40^\circ$  temporal. We have, however, chosen for two reasons to present in the following table only the results obtained in the temporal meridian.

(a) The results obtained in this meridian demonstrate sufficiently well all the facts that need be taken into consideration. Space will not, therefore, be given to the results for both meridians.

(b) The second point of our problem requires us to correlate the increased amount of induction caused by a given decrease of illumination with the change in the color limits it produces. The limits of color sensitivity can be more easily investigated in the temporal meridian because the sensitivity to some colors extends in the nasal region beyond the  $92^\circ$  point, which is the limit of measurement for the apparatus we used. This is true in particular in case of Observer C as may be seen in Table VI. Both purposes of the investigation are, then, better satisfied by results obtained in the temporal meridian.

The results show in general the following facts.

(1) The amount of induction increases with the distance from the fovea.

(2) The amount of induction increases with decrease of illumination.<sup>42</sup>

(3) The amount of induction from the white screen is greater than that from the black screen.<sup>42a</sup>

(4) The amount of increase of induction at decreased illumination is greater in case of the white screen than in case of the black screen.

(5) The white and black screens induce most across the stimuli that are farthest removed from them in brightness, and least across those which are nearest to them in brightness. That

<sup>42</sup> This statement is meant to apply only to the range of illumination worked with. The induction was not measured when the illumination was very low, nor when it was very intensive.

<sup>42a</sup> An exception to this statement of result occurs in case of gray No. 2 at  $40^\circ$ . This stimulus is so near to white in brightness that the induction across it, according to the principle stated in (5) above, is greater for the black screen than for the white.

is, the white screen induces more black across the gray of the brightness of blue than across the gray of the brightness of yellow; the black screen induces more white across the gray of the brightness of yellow than across the gray of the brightness of blue.

Results are given in detail in Tables XII and XIII. Table XII gives the results for observer *A* taken on the temporal meridian, and Table XIII, the results for Observer *C* for the same meridian. There is some difference in the amount of induction reported by the different observers, but since the preceding general statement of results is clearly borne out in every case, it is not deemed necessary to give space to results from all the observers used. In these tables, column 1 gives the degree of excentricity at which the observation was made; columns 2, 3, and 4, show respectively the stimulus used, and the amounts of induction from the white and from the black screens at standard illumination. Columns 5, 6, and 7, give the same data for decreased illumination.

TABLE XII.

*A. Showing the amount of contrast induced by the white and the black screens at standard and decreased illumination upon the grays of the brightness of the colored stimuli at standard and at decreased illumination.*<sup>43</sup>

Fixation	Standard illumination			Decreased illumination		
	Stimulus (gray of brightness of each of the four colors at standard illumination)	Amt. induc- tion of white screen	Amt. induc- tion of black screen	Stimulus (gray of brightness of each of the four colors at decreased illumination)	Amt. induc- tion of white screen	Amt. induc- tion of black screen
25°	gray no. 2	Black 135°	White 110°	gray no. 2	Black 220°	White 170°
	gray no. 8	" 155°	" 60°	gray no. 6	" 270°	" 80°
	gray no. 24	" 230°	" 28°	gray no. 41	" 320°	" 40°
	gray no. 37	" 290°	" 12°	gray no. 20	" 330°	" 30°
40°	gray no. 2	" 200°	" 300°	gray no. 3	" 320°	" 360°
	gray no. 8	" 300°	" 132°	gray no. 5	" 360°	" 180°
	gray no. 24	" 360°	" 60°	gray no. 50	" 360° <sup>44</sup>	" 0°
	gray no. 29	" 360°	" 28°	gray no. 13	" 360°	" 100°

<sup>43</sup> It is obvious that the method used in this and the following tables of expressing the amount of brightness induction gives an underestimation.

TABLE XIII.

*Observer C.*

25°	gray no. 2	Black 70°	White 55°	gray no. 2	Black 130°	White 70°
	gray no. 8	" 84°	" 48°	gray no. 6	" 155°	" 59°
	gray no. 24	" 93°	" 30°	gray no. 40	" 187°	" 45°
	gray no. 37	" 160°	" 15°	gray no. 17	" 244°	" 22°
40°	gray no. 2	" 110°	" 200°	gray no. 3	" 216°	" 340°
	gray no. 7	" 142°	" 160°	gray no. 4	" 230°	" 320°
	gray no. 24	" 180°	" 95°	gray no. 50	" 360° <sup>44</sup>	" 0°
	gray no. 29	" 214°	" 35°	gray no. 7	" 300°	" 108°

*Results when the gray screen matching the colored stimulus in brightness at standard illumination is used.* It was necessary to perform the experiments bearing on this point at decreased illumination only. For them the campimeter screens which matched in brightness the four principal colors of the Hering series at standard illumination served as inducing surfaces. For

Suppose, as is shown in Table XII, that No. 24 Hering gray has been darkened by induction until it matches in brightness a disc made up of 230° of black and 130° of the No. 24 gray. The amount of induction is greater than is represented by the 230° of black because the induction has not lessened the amount of light coming to the eye from the gray paper while the addition of 230° of black to the measuring-disc has cut off approximately  $\frac{2}{3}$  of the light coming from the gray paper. That is, in the one case enough black has been added by induction to reduce 360° of No. 24 gray to the given point in the brightness scale, while in the other enough black was added by direct mixing to lower only 130° of No. 24 gray to this point in the scale. Moreover, the underestimation will be increased by this method of measuring in proportion as the amount of induction is increased because the greater the induction is the more black and the less gray will have to be used in the measuring-disc. All that can be said accurately is that a certain gray darkened or lightened by induction matches in brightness a gray made up of a certain amount of the given gray plus a certain amount of black or white. The exact amount of the induction can not be separated out. Further just because the brightness added by contrast does not alter the amount of light coming to the eye while the brightness added in any method of measurement does change this amount of light, the writer knows of no way by which an exact expression can be obtained. The method she has used, however, does serve as a means of comparing the amounts of induction occurring under different conditions sufficiently accurately for her purpose at this point.

<sup>44</sup>The gray No. 50 was in reality rendered blacker by the inductive action of gray No. 24 than the Hering black we used on the measuring-disc. A match thus could not be attained with black 360° as the table indicates.

the contrast surfaces, grays of the brightness of these colors at decreased illumination were chosen. The methods of measuring, precautions in working, parts of the retina investigated, etc., were the same as in the preceding determinations. The following general statement of results may be made.

1. At the  $25^\circ$  point the brightness of yellow was found not to have changed at all with the decrease of illumination produced by changing the illumination from the value selected as standard to the value selected for the comparison; the brightness of green lightened by an amount equal to the difference between No. 8 and No. 6 of the Hering series of grays; red darkened by an amount equal to the difference between No. 24 and No. 40; and blue lightened by an amount equal to the difference between No. 32 and No. 20. The amount of induction by the gray screen of the original brightness of the color upon the gray stimulus of the brightness of the color as altered by the decreased illumination, expressed in terms of Hering white and black, was for yellow  $0^\circ$ , for green  $60^\circ$  of white, for red  $27^\circ$  of black, and for blue  $20^\circ$  of white.

2. At the  $40^\circ$  point, the yellow darkened by an amount equal to the difference between No. 2 and No. 3 of the Hering grays; green lightened by an amount equal to the difference between No. 8 and No. 5; red darkened by an amount equal to the difference between No. 28 and No. 50; and blue lightened by an amount equal to the difference between No. 28 and No. 13. The amount of induction produced by these changes was for yellow  $280^\circ$  of black, for green  $130^\circ$  of white, for red  $360^\circ$  of black, and for blue  $60^\circ$  of white. These results are shown in detail in Table XIV.

*(B.) The Effect of These Amounts of Induction upon the Limits of Color Sensitivity.*

In order to obtain an estimate of the range of effect upon the limits of color sensitivity of the induction of the screens at standard and at decreased illumination, the breadth of the color zones was determined at both illuminations (*a*) when white and black served in turn as campimeter screens; and (*b*) when a gray matching the color in brightness at standard illumination was

TABLE XIV.

A. *Showing the amount of contrast induced at decreased illumination on grays of the brightness of the colors at decreased illumination by the gray screens matching the colors in brightness at standard illumination.*

Fixation	Stimulus	Screen	Amount of Induction
25°	gray no. 2	gray no. 2	0
	gray no. 6	gray no. 8	white 60°
	gray no. 41	gray no. 24	black 27°
	gray no. 20	gray no. 37	white 20°
40°	gray no. 3	gray no. 2	black 280°
	gray no. 5	gray no. 8	white 130°
	gray no. 50	gray no. 24	black 360° <sup>45</sup>
	gray no. 13	gray no. 29	white 60°

used. The preëxposure was in each case to gray of the same brightness as the stimulus at the illumination used.

*Results when white and black screens were used.* When the stimulus color is gotten by reflection from a pigment surface, two factors operate to give a change of result when the illumination is decreased. (1) There is a decrease in the amount of colored light coming to the eye. (2) There is an increase in the inductive action of the screen due to the change in the brightness relation of the stimulus to screen and to the increased sensitivity of the eye to brightness contrast at decreased illumination.

In order to find out how much of our results with the white and black screens should be attributed to the decrease in the amount of colored light coming to the eye produced by the decreased illumination, and how much to the increased inductive actions of the screens, the limits of sensitivity were also determined at both illuminations with the screens of the gray into which the color disappears in the peripheral retina. From the values obtained with the three screens at both illuminations, the amount of change due to decrease in the amount of colored light coming to the eye and the amount due to induction by the white and black screens were calculated as follows. (a) From

<sup>45</sup> The gray No. 50 was in reality rendered blacker by the inductive action of gray No. 24 than the Hering black we used on the measuring-disc. A match could not be thus attained with black 360° as the table indicates.

the number of degrees expressing the limits for a given color at standard illumination with a screen of the brightness of the color at that illumination was subtracted the number expressing its limit at decreased illumination, with a screen of the brightness of the color at the decreased illumination. That this gave the number of degrees the zone of sensitivity was narrowed by the decrease in the energy of the stimuli, may be said with the following qualification. If there is any influence upon color sensitivity of the local brightness-adaptation of the retina produced by the change in the general illumination, it is, of course, included in this effect. But, since this influence would have to be brought about by previous exposure to the illumination in question, it can be reduced to a minimum by guarding against an exposure to it for any considerable length of time. The effect of whatever adaptation there may be, however, can not be isolated or separated out from the above result, and the value expressing the amount the limit is narrowed by the actual decrease of the energy of colored light coming to the eye cannot, strictly speaking, be obtained. But it is probable that the adaptation effect is not sufficiently strong to influence the limits, since the sensitivity of the extreme peripheral retina falls off very abruptly from point to point. The difference, then, between the color limit obtained at standard illumination and the limit at decreased illumination, when in both cases there is no brightness induction from the screen, may be said to approximate the effect upon the limits produced by the decrease in the amount of colored light coming to the eye. (*b*) Figures can be obtained, however, from our results, which express the amount by which the zones are narrowed by the change in the inductive action of the white and black screens produced by decreasing the illumination, that are not open to theoretical questioning; for the influence of local brightness-adaptation, if there be any, is a constant for all screens at the same illumination. If then, the number of degrees which expresses the limits of sensitivity for either the white or the black screen at decreased illumination is subtracted from the number expressing the limit with a screen of the gray of the brightness of the color at this illumination, the result will rep-

resent the extent to which the limit was narrowed by the action of induction alone.

The results show in general the following facts:

1. At standard illumination, induction from the white screen narrows the limits of yellow and red; induction from the black screen narrows the limits of blue and green. The difference is in no case more than  $4^{\circ}$ .

2. At decreased illumination, the induction from the white screen narrows the limits of all the colors much more considerably than does the induction from the black screen.<sup>46</sup>

3. The values expressing the narrowing of the limits caused by decrease of illumination without induction, are greatest in case of those colors which undergo maximum change of brightness in passing into the periphery, namely, for blue and red.

We have shown by the results of the preceding section, that the increased induction produced by decrease of the general illumination is greater for the white screen than for the black, and, by the results of this section, that this increase is effective to the extent of narrowing the limits of sensitivity to all colors from  $5^{\circ}$  to  $13^{\circ}$  with this screen. With the black screen, the limits were narrowed from  $0^{\circ}$  to  $6^{\circ}$ . At standard illumination, the limits were narrowed only from  $1^{\circ}$  to  $4^{\circ}$  with either the white or the black screen.

Results in detail are given in Tables XV and XVI taken from the temporal meridians of the observers whose observations are recorded in Tables XII and XIII. In column 1, Tables XV and XVI, is given the stimulus. Column 2 shows the limit of sensitivity to the stimulus at standard illumination with a screen of a gray of the brightness of the color at standard illumination; column 3 shows the limit with a white screen; and column 4 with a black screen. Column 5 shows the limit at decreased illumination with a screen of the brightness of the

<sup>46</sup>For Observer *A* the results for green present an exception. At the decreased illumination used the green stimulus appeared bluish in the central retina. The induction of the black screen caused it to appear as a pale blue at a comparatively slight degree of excentricity. According to our definition of color limit, this point is the limit of green. It is, however, obvious that the exception is due rather to the qualitative than to the quantitative effect of brightness upon color.

color at decreased illumination; column 6 shows the limit with a white screen; and column 7 with a black screen.

TABLE XV.

A. *Showing the color limits at standard and decreased illumination (a) with gray screens of the brightnesses of the colors at the illumination used; and (b) with white and black screens.*

Stimulus	Standard Illumination		Decreased Illumination			
	Limit with gray screen of brightness of color at standard illumination	Limit with white screen	Limit with black screen	Limit with gray screen of brightness of color at decreased illumination	Limit with white screen	Limit with black screen
Yellow	44°	42°	45°	43°	35°	43°
Green	37°	36°	34°	36°	31°	27°
Red	43°	42°	44°	40°	31°	40°
Blue	53°	50°	49°	49°	36°	43°

TABLE XVI.

*Observer C.*

Yellow	49°	46°	50°	46°	36°	44°
Green	44°	42°	40°	41°	28°	33°
Red	45°	41°	45°	41°	34°	41°
Blue	56°	55°	53°	50°	38°	44°

Tables XVII and XVIII to show the following facts:

(a) How much the decrease of illumination narrowed the limits of color sensitivity by causing a decrease in the energy of the light-waves coming to the eye. This was determined by subtracting the value of the limit at decreased illumination with the screen of a gray of the brightness of the color at decreased illumination from its value at full illumination with the gray screen of the brightness of the color at full illumination. (b) How much the limits were narrowed by the action of the white and black screens at decreased illumination. This was ascertained by subtracting the values of the limit with the white and the black screen at decreased illumination from the value of the limit at decreased illumination with the gray screen of the brightness of the color at this illumination. (c) How much

more the limits were narrowed by the white and the black screens at decreased than at full illumination. This was computed for the white screen, for example, as follows: The quantity, limit at decreased illumination for gray screen of brightness of color at decreased illumination, minus limit for white screen at decreased illumination, is subtracted from the quantity, limit at full illumination for gray screen of brightness of color at full illumination minus limit for white screen at full illumination. A similar computation was made for the black screen.

TABLE XVII.

*A. Showing (a) how much the limits were narrowed by decrease in the amount of colored light coming to the eye; (b) how much they were narrowed by increased induction of white and black screens at decreased illumination; and (c) how much more they were narrowed by induction of white and black screens at decreased than at full illumination.*

Stimulus	How much limits were narrowed by decrease in amount of colored light coming to the eye	How much limits were narrowed by induction of white screen	How much limits were narrowed by induction of black screen	How much more limits were narrowed by white screen at decreased than at full illumination	How much more limits were narrowed by black screen at decreased than at full illumination
Yellow	1°	8°	0°	6°	1°
Green	1°	5°	9°	4°	6°
Red	3°	9°	0°	8°	1°
Blue	4°	13°	6°	10°	2°

TABLE XVIII.

*Observer C.*

Yellow	3°	10°	2°	7°	3°
Green	3°	13°	8°	11°	4°
Red	4°	7°	0°	3°	0°
Blue	6°	12°	6°	11°	3°

*Results when a gray screen matching the color in brightness at standard illumination is used.* In these experiments a determination was made of the amount the limits of sensitivity are changed by the brightness induction caused by the alteration of

the brightness relation between stimulus and screen with decrease of illumination, when a screen is used which matches the color in brightness at standard illumination. This determination was made as follows.

An estimate was made of the amount the limits were narrowed by decrease of illumination when a screen of the brightness of the color at standard illumination is used for both standard and decreased illuminations. From this result was subtracted the amount the limits were narrowed by decrease of illumination when the screen is made in turn of the brightness of the color at standard and at decreased illumination. The difference obtained represents the value sought. It is given in Table XIX.

TABLE XIX.

*A. Showing how much the color limits were narrowed at decreased illumination by the induction of the screen which matched the color in brightness at standard illumination.*

Stimulus	Screen of brightness of color at decreased illumination	Limit	Screen of brightness of color at standard illumination	Limit	Amount limit was narrowed by change in brightness relation between stimulus and screen caused by decreased illumination
Yellow	gray no. 3	43°	gray no. 2	41°	2°
Green	gray no. 5	36°	gray no. 8	29°	7°
Red	gray no. 50	40°	gray no. 24	33°	7°
Blue	gray no. 13	49°	gray no. 28	46°	3°

*(C.) The Effect of These Amounts of Induction upon the Limens of Color at Different Degrees of Excentricity.*

We have shown the effect of decreasing the general illumination upon the color sensitivity of the peripheral retina with gray, white, and black screens by the effect on the limits of sensitivity. This is only an indirect means of estimating its influence, for the results obtained cannot be translated into terms of direct measurement, owing to the irregular decrease in sensitivity of

the peripheral retina from the fovea outwards. In this section, we shall measure the influence of changes of illumination directly by the changes produced in the limen of sensation at various angles of excentricity. As in the previous section, measurement will be made of the effect upon sensitivity (*a*) of the decrease in the amount of colored light coming to the eye, produced by the decrease of illumination, (*b*) of the difference in the inducing power of the white and black screens, and (*c*) of the change in the brightness relation of stimulus to background.

To determine the first of these three points, a campimeter screen had to be selected that gave no brightness contrast with the stimulus. To provide for differences in the brightness of the colors at the different points observed for the two illuminations at which we worked, a preliminary determination of the brightness of the sensation at these points was made at both illuminations by the flicker method. The brightness of the screen was chosen in each case of the brightness of the color according to these determinations. To eliminate the effect of preëxposure, the stimulus previous to exposure was in every case covered by a gray of the brightness of the color for the illumination used at the point of the retina at which we were working. Thus no brightness after-image was carried over to exert an inhibitive action upon the color sensation. The stimulus was a disc compounded of the sectors of the color, and of the gray of the brightness of the color for the illumination used at the point of the retina under investigation. The proportions of the sectors were altered until the observer gave the judgment of just noticeable color. The average of judgments made in ascending and descending series was chosen as the final value of the limen. The difference between the limens at standard and decreased illumination was taken as the measure of the loss in intensity which the stimulus had sustained by the decrease of illumination.

The effect upon the color limen of the increased induction from the white and black screens was shown by the same method, with the exception that the white and black screens were substituted for the gray of the brightness of the color. The stimulus was a disc composed of sectors of color and gray of the brightness of

the color at the angle of excentricity at which the determination was made.

The effect of the change in the brightness relation between the stimulus color and the screen produced by decrease of illumination was shown as follows. An estimate was made of the amount the limens are raised by the decrease of illumination when a screen was used for both standard and decreased illumination that had a brightness-value equal to the color at standard illumination. From these results was subtracted the amount the

TABLE XX.

*A. Showing how much the limens of sensitivity were raised at the fovea, and at points 15°, 25°, 30° from the fovea in the horizontal meridian on the temporal side by the decrease in the amount of colored light coming to the eye produced by the decrease in the general illumination.*

Stimulus	Point on horizontal temporal meridian at which limen was taken	Limen at standard illumination with screen of brightness of color at standard illumination	Limen at decreased illumination with screen of brightness of color at decreased illumination	How much limen was raised at decreased illumination
Yellow	0°	18°	20°	2°
	15°	22°	32°	10°
	25°	35°	40°	5°
	30°	50°	65°	15°
Green	0°	20°	20°	0°
	15°	27°	28°	1°
	25°	40°	50°	10°
Red	0°	9°	11°	2°
	15°	9°	13°	4°
	25°	17°	25°	8°
	30°	25°	45°	20°
Blue	0°	9°	10°	1°
	15°	10°	13°	3°
	25°	12°	15°	3°
	30°	20°	40°	20°

limens were raised by decreasing the illumination when the screens were made in turn of the brightness of the color at standard and at decreased illumination. The difference obtained represents the value sought. These results are of particular importance because they show that the influence of the brightness of the surrounding field can not be eliminated even when a screen of the brightness of the color is used unless some means be had of maintaining the general illumination of the room constant.

Table XX shows how much the limens of sensitivity were raised at the fovea and at points  $15^\circ$ ,  $25^\circ$ , and  $30^\circ$  from the

TABLE XXI.

*A. Showing the color limens at standard and decreased illuminations with white and with black screens.*

Stimulus	Point on horizontal temporal meridian at which limen was taken	White screen		Black screen	
		Limens at standard illumination	Limens at decreased illumination	Limens at standard illumination	Limens at decreased illumination
Yellow	$0^\circ$	$22^\circ$	$25^\circ$	$28^\circ$	$30^\circ$
	$15^\circ$	$25^\circ$	$50^\circ$	$35^\circ$	$45^\circ$
	$25^\circ$	$50^\circ$	$80^\circ$	$65^\circ$	$85^\circ$
	$30^\circ$	$80^\circ$	$125^\circ$	$95^\circ$	$113^\circ$
Green	$0^\circ$	$22^\circ$	$25^\circ$	$28^\circ$	$30^\circ$
	$15^\circ$	$30^\circ$	$36^\circ$	$35^\circ$	$43^\circ$
	$25^\circ$	$50^\circ$	$75^\circ$	$75^\circ$	$220^\circ$
Red	$0^\circ$	$13^\circ$	$20^\circ$	$10^\circ$	$14^\circ$
	$15^\circ$	$19^\circ$	$35^\circ$	$15^\circ$	$21^\circ$
	$25^\circ$	$30^\circ$	$55^\circ$	$23^\circ$	$35^\circ$
	$30^\circ$	$50^\circ$	$330^\circ$	$29^\circ$	$58^\circ$
Blue	$0^\circ$	$17^\circ$	$22^\circ$	$10^\circ$	$12^\circ$
	$15^\circ$	$25^\circ$	$40^\circ$	$12^\circ$	$17^\circ$
	$25^\circ$	$35^\circ$	$60^\circ$	$18^\circ$	$25^\circ$
	$30^\circ$	$40^\circ$	$90^\circ$	$30^\circ$	$60^\circ$

fovea in the horizontal meridian on the temporal side by the decrease in the amount of colored light coming to the eye pro-

duced by the decrease in the general illumination. The results of this table may be generalized as follows:

1. The limen of color is higher in the periphery than in the center of the retina at both illuminations.

2. The limen of color is higher at decreased illumination than at standard illumination.

3. The direct effect upon the intensity of the sensation produced by decreasing the illumination is shown by the limen determinations to be inconsiderable. In the central retina, the difference is but  $1^\circ$  or  $2^\circ$ . In the peripheral retina at the points considered there is a difference of from  $10^\circ$  to  $20^\circ$ .

Table XXI shows the color limens at both standard and decreased illuminations when white and black screens are used, at the fovea, and at points  $15^\circ$ ,  $25^\circ$ , and  $30^\circ$  in the horizontal meridian on the temporal side.

Table XXII has been compiled from Tables XX and XXI to show how much greater the limens were for white and black screens at decreased than at full illumination; how much of the effect may be ascribed to the reduction of the amount of colored light coming to the eye; and how much to the increased induction of the screens. It will be seen from the results of this table that the loss of the sensation in intensity due to the increased brightness induction is much greater than that caused by the reduction in the amount of colored light coming to the eye.

It was shown in Table XIV that quite a great deal of brightness induction is caused by the change in brightness relation between color and screen produced by decreasing the illumination. Table XIX shows how much this induction narrows the limits of sensitivity to the four colors used. Table XXIII, shows how much the limens are raised when the illumination is decreased by the inductive action caused by the change in the brightness relation between stimulus color and gray screen of the brightness of the color at standard illumination.

TABLE XXII.

*A. Showing how much greater the limens were with white and black screens at decreased than at standard illumination and how much of this effect may be ascribed to the reduction in the amount of colored light coming to the eye and how much to the increased inductive action of the screens.*

Stimulus	Point on horizontal meridian at which limen was taken	White screen			Black screen		
		Total amount greater	Amount due to decrease in amount of colored light coming to eye	Amount due to increase of induction of screen	Total amount greater	Amount due to decrease in amount of colored light coming to eye	Amount due to increase of induction of screen
Yellow	0°	7°	2°	5°	12°	2°	10°
	15°	28°	10°	18°	23°	10°	13°
	25°	45°	5°	40°	50°	5°	45°
	30°	75°	15°	60°	63°	15°	48°
Green	0°	5°	0°	5°	10°	0°	10°
	15°	9°	1°	8°	16°	1°	15°
	25°	35°	10°	25°	180°	10°	170°
Red	0°	11°	2°	9°	5°	2°	3°
	15°	26°	4°	22°	12°	4°	8°
	25°	38°	8°	30°	18°	8°	10°
	30°	305°	20°	285°	33°	20°	13°
Blue	0°	13°	1°	12°	3°	1°	2°
	15°	30°	3°	27°	7°	3°	4°
	25°	48°	3°	45°	13°	3°	10°
	30°	70°	20°	50°	40°	20°	20°

TABLE XXIII.

A. Showing how much the color limens were raised at decreased illumination by the induction of the screens which matched the color at standard illumination.

Stimulus	Point on horizontal temporal meridian at which limen was taken	Limens with screen of brightness of color at decreased illumination	Limens with screen of brightness of color at standard illumination	Amount limen was raised by change in brightness relation between stimulus and screen caused by decrease of illumination
Yellow	0°	20°	20°	0°
	15°	32°	32°	0°
	25°	40°	40°	0°
	30°	65°	116°	51°
Green	0°	20°	20°	0°
	15°	28°	40°	12°
	25°	50°	190°	140°
Red	0°	11°	11°	0°
	15°	13°	24°	11°
	25°	25°	48°	23°
	30°	45°	150°	105°
Blue	0°	10°	12°	2°
	15°	13°	16°	3°
	25°	15°	23°	8°
	30°	40°	55°	15°

(D.) The Influence of Change of Illumination upon the Action of the Preëxposure on the Limens and Limits of Color.

The brightness of the preëxposure exerts an influence upon the color observation because the eye carries over an after-image from the preëxposure into the color observation. If, for example, the preëxposure is to black, a white after-image is aroused which fuses with the succeeding color sensation and strongly reduces its saturation. The effect of preëxposure is especially strong in the peripheral retina because a very strong brightness after-image is aroused in the peripheral retina by a very short period of stimulation. It is very difficult for the writer to predict from the data she has at hand with regard to the effect of change of illumination upon the sensitivity of the

peripheral retina to the brightness after-image just what will be the effect of change of illumination upon the action of pre-exposure on the color sensitivity of the peripheral retina. But even though there be no change in the sensitivity of the peripheral retina to the brightness after-image with change of illumination, it is obvious that there will be some effect of the change of illumination because of the change in the brightness relation of the preëxposure card to the colored stimulus. In case the stimulus light is gotten by reflection from pigment surfaces, this change of brightness relation is due to the shift in the brightness of the colors produced by the change in the illumination. In case transmitted light is used as stimulus, the brightness of the stimulus color is independent of changes in illumination and will remain constant; but a change in the brightness relation of stimulus to preëxposure will occur because the pre-exposure will lighten or darken with change of illumination. The writer hopes to make the quantitative investigation of this point the subject of a future study. At present she can only point out that if a guarantee is wanted that the effect of the brightness of the preëxposure is eliminated from the results of the observation, the preëxposure must be to a gray of the brightness of the color and the illumination of the room must be kept constant.

The foregoing results show how strongly the changes in the illumination of the visual field influence the color sensitivity of the peripheral retina, particularly when the stimulus is surrounded by a white field. They also show that the influence of surrounding field can not be eliminated even by means of a campimeter screen of the brightness of the color unless some means be had of keeping the general illumination of the room constant. It is obvious without further comment how important it is that a method be devised to standardize this factor. The preceding experiments indicate that without this standardization, no experiment can be repeated from time to time under the same conditions relative to any one of the brightness factors that influence color sensitivity. Results thus obtained are far from comparable.

*E. Methods of Standardizing These Factors.*

We have shown in the preceding analysis of the color observation that the factors which influence the limits of color sensitivity, and which, therefore, require standardization, are the brightness of the surrounding field, the brightness of the pre-exposure, and the general illumination of the retina. Standard conditions require either that the influence of a factor be eliminated, or that it be reduced to a constant. We have been able to treat all three of these variable factors in one or the other of these two ways. The effect of preëxposure and campimeter screen has been eliminated and methods of measuring the general illumination and of keeping it constant have been provided.

As we have seen, the surrounding field influences the color sensation by adding brightness to the stimulus by induction. When, for example, the surrounding field is black, white is induced by contrast across the stimulus color. Since the colors all differ in brightness, the induction takes place in different amounts for the different colors. This white in proportion to its amount reduces the action of the colors on the retina. Further, a given amount of white affects to different degrees the action of the different colors on the retina. The influence of pre-exposure is even more important than of surrounding field. If the preëxposure is to black, white is added as after-image to the stimulus color. The effect of a black preëxposure upon the stimulus color is greater than the effect of a black surrounding field because more white is added as after-image of preëxposure than is induced by contrast from the surrounding field. Now, since brightness induction is greatest when there is maximal opposition between the inducing and induced fields, and since the brightness after-image also is most intensive when there is maximal opposition between the stimulus and the projection field, it is evident that no one screen nor preëxposure can be found that will influence each color by an equal amount. The black preëxposure and surrounding field concomitant upon work in the dark-room can be considered no exception to this statement. The influence of preëxposure and surrounding field can not be successfully eliminated in work in the dark-room. By

using one screen and preëxposure standard conditions of contrast induction and brightness after-image can be maintained only if the colors are made of equal brightness. The objections to this procedure were pointed out in an earlier section. There remains the alternative of choosing in each case gray papers of the brightness of the colored stimulus for the screen and the preëxposure. This necessitates changes of screen and of preëxposure for each stimulus, but insures the complete elimination from the color excitation of all brightness influence due either to preëxposure or to stimulation of the surrounding retina. In this way alone, then, may a proper regulation of these factors be obtained for any investigation whatsoever of the sensitivity of the peripheral retina. Further, the method gives a proper basis with regard to these two important factors from which to start all investigations of the effect of achromatic conditions upon color sensitivity.

Standardization for either one of these factors, however, can be accomplished for one degree of illumination only. As the general illumination changes, the relation of the brightness of the preëxposure and of the surrounding field to the brightness of the colored stimulus changes.<sup>47</sup> It is obvious, then, that if standardization is to be accomplished with regard to the influence of either of these factors, some means must be devised of maintaining the general illumination of the retina constant.

In order to obtain a standard illumination, two things are necessary: (a) A means of controlling the illumination must be provided, which is sufficiently sensitive to cause small changes. (b) A method of measuring the illumination produced has to be devised; at least, a means must be secured for determining when an illumination has been obtained that is equal to a given preceding illumination. We shall first discuss the method of measurement we adopted. As stated earlier in our paper, no

<sup>47</sup> When the colored light used to stimulate the retina is independent of the general illumination, e.g., when it is obtained from the spectrum, from monochromatic sources, or from standard filters, these two factors alone will modify the result of the color observation. If, however, light reflected from a pigment surface be used as stimulus, a change in the illumination will in addition change the amount of colored light coming to the eye.

satisfactory means of determining the amount of daylight illumination in a room has been provided by the physicist, so there is little hope at this time of solving the problem from that side. The brightness induction of the peripheral retina, however, has been found by us to be extremely sensitive to changes in the general illumination. This phenomenon seems to provide us with a sensitive measure of these changes, while, at the same time, it represents the combined effects for sensation of the principal subjective factors that might vary from day to day. To apply the method in its most sensitive form, the inductive power of white was chosen because it is the most strongly affected by illumination changes. For example, when No. 14 Hering gray was used as stimulus and white as campimeter screen, a noticeable change was produced in the induction when the white curtain of the optics-room was pulled forward 1 cm. from a position in which its edge was directly above the long axis of the campimeter. This caused a change in the illumination of the room so small that it could not be directly sensed. Further, at 11 o'clock in the morning of a bright day in September, when a point at  $25^\circ$  on the nasal meridian was stimulated, Observer *A* reported that the white screen induced black across the stimulus No. 14 gray to an amount that caused it to equal in brightness  $107^\circ$  of black and  $253^\circ$  of No. 14 gray; at 2 o'clock of the same day the induction was increased until the No. 14 gray matched  $150^\circ$  of black and  $210^\circ$  of the gray; at 4 o'clock of the same day the No. 14 gray matched  $180^\circ$  of black and  $180^\circ$  of the gray.<sup>48</sup> Working at  $25^\circ$  in the temporal meridian, this observer reported at different times during one day and on different days, the wide variations shown by the following figures:  $283^\circ$  of black,  $225^\circ$ ,  $145^\circ$ ,  $190^\circ$ ,  $238^\circ$ , etc. Observer *C* reported less induction, but her variations from time to time were equally great. At  $25^\circ$  in the temporal meridian, she found at different times  $80^\circ$  of black,  $103^\circ$ ,  $160^\circ$ ,  $175^\circ$ , etc. After a careful study of the phenomenon with different screens and with different stimuli, the inductive action of the white screen upon a stimulus of No. 14 Hering gray, at  $25^\circ$  in the

<sup>48</sup> This increase in the inductive action of the screen caused by the decrease in illumination, was accompanied by a shrinkage of the zones sensitive to color covering an area of 4 to 6°.

temporal meridian, was found to provide the best means of detecting changes in the illumination of the optics-room. At this point on the retina, the induction was by no means minimal, nor was it sufficiently great to cause the medium gray chosen for our stimulus to appear too dark to give a small j. n. d. of sensation.

The sensitivity of this method of detecting changes in the general illumination was compared with the sensitivity of the Sharpe-Millar portable photometer. In this photometer one of the comparison fields is illuminated by the light of the room and the other by a standard tungsten lamp enclosed in the photometer box. When the room is illuminated by daylight, the field receiving the light of the room is seen as white, while the field lighted by the tungsten lamp appears as a saturated orange. The difference in color between the two fields renders the photometric judgment difficult and makes the instrument very insensitive for daylight tests. For example, our tests showed that by the method for indentifying an illumination described in the text, a change in illumination could be detected which was produced by drawing the white curtain 1 cm. from a position in which its edge was directly above the long axis of the campimeter. But with the receiving surface of the portable photometer in precisely the same position as the stimulus screen of the campimeter, the edge of the curtain had to be moved 11.3 cm. in order that the change of illumination might be detected. Moreover, this amount of change could be detected only in case the photometric field was continuously observed while the curtain was being drawn, in which case the comparison field was observed to become slightly darkened. The judgment was made, then, in terms of a just noticeably different brightness of the field which was illuminated by the daylight, rather than in terms of a disturbance in the brightness-equality of the two fields. When, on the other hand, the judgment was made in terms of a just noticeable disturbance in the equality of the two fields, as the judgment would have to be made if the photometer were to be employed for the reproduction of any former illumination taken as standard, the curtain had to be drawn 44.2 cm. before the change could be detected. This j. n. d. represents an amount of illumination equal to 2.5 foot-candles.

The next step was to procure a means of changing the illumination of the room by very small amounts. This was accomplished by drawing the white curtain (described p. 86) across the skylight above the apparatus. The drawing of this curtain several inches made little difference in the illumination directly observable by the eye, although, as we have said, a change of 1 cm. when the edge of the curtain was directly above the apparatus, produced a noticeable change in the inductive action of the white screen.

Having thus provided ourselves with a means of producing

small changes of illumination and with a method of detecting them, we had in order to complete our work but to choose an illumination for each observer, which could be taken as standard. Since we wished to work on both light days and days of medium darkness, an average had to be chosen as our standard from the measurements obtained on a number of days ranging from light to dark, so that on bright days the room could be darkened, and on dark days it could be lightened until this value was obtained. For Observer *A* an illumination was selected which caused an induction of black across No. 14 gray stimulus viewed at  $25^\circ$  in the temporal meridian to an amount which caused the gray stimulus to equal in brightness  $210^\circ$  of black and  $150^\circ$  of No. 14 gray; for Observer *B*  $180^\circ$  of black and  $180^\circ$  of No. 14 gray; and for Observer *C*  $145^\circ$  of black and  $215^\circ$  of No. 14 gray. The amount of black induction was identified in each case by means of a measuring-disc made up of sectors of black paper and No. 14 gray of the Hering series.

Previous to each series of observations the illumination of the room was changed until the amount of brightness induction was brought to the value chosen as standard. It was tested at intervals during the sitting and was readjusted when necessary. Details of the method of doing this are as follows: When the white screen and the No. 14 gray stimulus had been put in place, the observer took his position and adjusted the fixation-knot in front of the motor for the  $25^\circ$  point on the temporal meridian. The measuring-disc set at the standard value was mounted on the motor. The observer reported whether the stimulus appeared lighter or darker than the measuring-disc, or of a brightness equal to it. If the judgment lighter or darker was given, the curtain was drawn one way or the other until the stimulus accurately matched the measuring-disc in brightness.

This method not only gives a sensitive measure of the changes of illumination of the visual field and a successful means of standardizing the illumination of a room by daylight, but it has in addition advantages for work in psychological optics not possessed by an objective standardization, could that be successfully obtained. The problem of standardization, includes more for the

psychologist than it does for the physicist, for the former has variables to take into account in addition to the changes that may take place in the energy of the stimulus. Even though the illumination of the room be made objectively constant, we should expect variations in the response of the retina to this illumination because of its own changes from time to time. Brightness contrast, for example, might be expected to vary from sitting to sitting even when the stimulus conditions are kept absolutely constant. Two factors would be concerned in these variations: changes in the inducing power of the surrounding parts of the retina, and changes in the sensitivity of the local area. These changes would take place even when the usual precautions known to the experimenter in this field have been observed. Such precautions are commonly limited to fatigue, adaptation, etc. These precautions do not provide for the changes that occur in the retina from day to day. Moreover, they do not adequately guard against a change in a factor, unless some measure of that factor be had. So far as the writer knows, in these general precautions intended to keep the state of the retina constant, no measure of the variable factor has been provided to test the adequacy of the method. The method proposed by us, however, is planned with this in view. It takes into account not only the objective, but the subjective variables, and reduces both to a constant. For example, when No. 14 gray surrounded by the white field is made equal to the measuring-disc composed of  $210^{\circ}$  of black and  $150^{\circ}$  of the No. 14 gray for Observer *A*, it means that the observation may be begun with the assurance that the total result of all the factors—the illumination of the room, the local sensitivity of the retina, and the inductive action of the surrounding parts of the retina—is the same as in the preceding observation.

What has just been said should not be considered as more than a general statement of the application of the principles of the method. In actual practice a greater refinement of working may be attained. If, for example, one wishes to use a preexposure differing in brightness from that of the colored stimulus, and doubts whether a test which covers only the local sensitivity of the retina and the inductive action of the surrounding parts is a

sufficient check upon the after-image sensitivity, he may make his standard include the effect of the preexposure he wishes to use. In short, if he does not consider adequate the more general test we have described, he may duplicate, in establishing his standard, any combination of brightness factors, due to preexposure, brightness of screen, or what not, that he may wish to use in his experiment proper.

The test of a method is how well it works. The test of this method is that we shall be able closely to duplicate our results from sitting to sitting regardless of the changes in the outside illumination from day to day or from morning until afternoon. The method stands the test. Long series of observations in the peripheral retina show a very small M. V.—much less even than is shown in the ordinary color observations in the central retina where, as compared with the peripheral retina, the factors extraneous to the stimulus exert little influence.

The following table has been compiled from a number of observations to show the variations in the results of color limens and color limits (*a*) when the general illumination was controlled according to the method described above, and (*b*) when no further precautions were observed than were used by previous investigators. In previous investigations of the color sensitivity of the peripheral retina, care has been taken to work only at the same hours of days that appeared equally bright, or, if on days of different brightness, to make a rough approximation of preceding illuminations by means of curtains without using either a definite standard or means of measuring. For our work with the illumination controlled, the gray of the brightness of the color at the illumination selected as standard was used for the preexposure and the campimeter screen. For the work without any especial control of the illumination, the gray of the brightness of the color on one of the days selected as typical was used throughout for preexposure and screen. This gave in the first case complete elimination of the effect of preexposure and surrounding field, and in the second case elimination as complete as could be gotten without accurate control of the general illumination. Results are given in the table for blue and green only because the sensitivity to these colors is affected most by changes of illumination.

Stimulus	Illumination	Screen and Preëxposure	Variation of limits on different days	Variation of limens on different days
Green	Controlled	Gray no. 8	0°	0° <sup>49</sup>
	Uncontrolled	Gray no. 9	4°-6°	60°-82° <sup>50</sup>
Blue	Controlled	Gray no. 28	0°	2°-3°
	Uncontrolled	Gray no. 30	4°-5°	18°-30°

At the conclusion of a piece of work the object of which has been the elimination of sources of error in one of the oldest and best developed fields of psychological investigation, the following comments having a more general application to other fields in which sensory determinations are required, may be justified. In all sensory determinations, investigators have been very much annoyed by the magnitude of the mean variation that has occurred in their results. This may be due to two sets of factors: errors in the control of the factors that influence the response of the sense-organ, and errors in judgment. To eliminate the latter source of errors, the psycho-physical methods have been devised. Before beginning her attempts to get a better control of the factors that influence the color sensitivity of the retina, the writer had used all the psycho-physical precautions known to her to eliminate errors in judgment, still her inability to reproduce her results rendered in her judgment any accurate investigation of the sensitivity of the peripheral retina hopeless. On the other hand, however, with the control she has been able to get of the factors that influence the sensitivity of the retina to color, and with only a casual observance of psycho-physical precautions, a very close reproduction of results has been rendered possible. With regard to work in the optics of color at least, then, she is forced to conclude that the major source of error is not in the factors that influence the judgment but in those that influence the response of the sense-organ. Moreover, she would suggest that if in other sensory fields more attention were paid to the factors

<sup>49</sup> The limen for green was taken in both cases at 25° on the temporal retina.

<sup>50</sup> The limen for blue was taken in both cases at 40° on the temporal retina.

that influence the response of the sense-organ and relatively less to the factors that influence the judgment, a higher degree of precision may be attained in our methods of working.<sup>51</sup>

<sup>51</sup> For a further discussion of this point, see Ferree, C. E., Transactions of the Illuminating Engineering Society, 1913, VIII.

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ARTHUR H. PIERCE, SMITH COLLEGE (*Bulletin*)

## Learning in Dementia Precox

By

EDWIN G. BORING

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A STUDY FROM THE PSYCHOLOGICAL LABORATORY OF THE  
GOVERNMENT HOSPITAL FOR THE INSANE,  
WASHINGTON, D. C.

With an Introduction by  
SHEPHERD IVORY FRANZ, PH.D.  
Scientific Director and Psychologist of the Government Hospital for the Insane

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## INTRODUCTION

Investigations of the factors influencing or effecting the training of the insane are intimately associated with the psychiatric problem of the occupation of patients. This problem is a practical one of great importance to hospitals for the insane, for upon its satisfactory solution depend many therapeutic procedures and financial readjustments. As long ago as 1830 occupation as a means of treatment of the insane received recognition in this country.<sup>1</sup> Although strictly controlled observations were lacking it was believed, for example, that an excited patient should be given employment, that his diffuse, and often destructive, movements would be replaced by those of practical value, and that, thereby, the random and apparently purposeless activity (and, *pari passu*, the mental abnormality) would give place to more normal conditions. It was also believed that the occupation of a melancholic patient would cause him to pay attention to the movements of his occupation, and that, in this way, the attention might be directed away from his fancied wrong-doings and his ills. In cases of primary dementia, so-called, employment was believed to be an effective method of retarding, if not of aborting or curing, the dementia. Coupled with these thera-

<sup>1</sup>The introduction of occupation as a therapeutic measure with the insane antedates this period by some years. Combe in 1831 refers to occupation as having been used in various institutions much earlier than this period. (See A. Combe: *Observations on mental derangement*. Edinburgh, 1831. Page 364.) An excellent account of an early view of the occupation of the insane is to be found in Prichard. (J. C. Prichard: *A treatise on insanity and other disorders affecting the mind*. Philadelphia, 1837.) In a footnote in that work (page 215) the conditions in the Richmond Lunatic Asylum, Dublin, Ireland, are cited, "where out of 277 patients, 130 are actively and usefully employed, *viz.*, 18 in garden labour, 16 in spinning, 12 in knitting, 18 at needle work, 12 in washing, 16 in carry coals, whitewashing the wards, tailoring and weaving, and 12 in learning to read." The last mentioned occupation, and others similar to it, might well be introduced or re-introduced into the hospitals for the insane, for upon the capability of the patient depends much of the success of the so-called after-care.

peutic beliefs the financial aspect of employment became prominent. In institutions for the insane there are multiple tasks of different characters which must be performed, and for which the institution must hire or supply workers. If some of these kinds of work can be satisfactorily performed by patients, there becomes available the money saved by reducing the amount for paid service, which can then be utilized for the purchase of more or better food and other comforts, in amusements and in a variety of other ways for the benefit of the patients. A decrease in absolutely necessary operating expenses may also be utilized to the advantage of the community by a reduction in the cost of maintenance. In many institutions at the present time this financial aspect of patient employment is apparently believed to be the more important. When, however, the insane are considered, as they should be, individuals who are ill and whose conditions require medical attention, it is the *therapeutic* aspect of occupation that should be predominant. It is not necessary that the therapeutic and financial aspects be separated entirely, for it is not only believable, but also probable, that the useful occupations, as therapeutic measures, are as beneficial as the so-called artistic occupations.

Although the belief is rather general that occupation is a therapeutic measure of value, recorded observations of the effects of different kinds of work on different classes of the insane are wanting. There are many articles and reports containing expressions of belief, and others of the nature of general discussion, and it is evident that in the selection of occupations and patients for the work much attention has been paid to local needs and individual preferences. A careful examination of psychiatric literature shows that in its scientific aspect the problem has scarcely been touched upon. Although the recording of casual observations in a matter of this kind is of value, this cannot take the place of experiment or of controlled observation. In this matter the rule of thumb should not be permitted to usurp the place of more accurate measuring instruments, for the problems from the standpoint of the hospital are therapeutic ones.

From what has been said it will be appreciated that the occupation of the insane must be considered from several angles if we

are to have adequate information for practical guidance. The more important of the angles from which to view the matter are the therapeutic or prescription angle, and the economic or financial angle. If the matter be looked at from one angle without considering others, there must result a loss to the patients because of this one-sided view, and it is only by a proper adjustment of observations from all angles that a correct and equitable solution of the complex problem or problems will be reached, and the patients thereby receive the greatest benefit.

The psychiatrist who has charge of a ward containing 100 patients, or of a hospital containing several thousand patients has a number of questions to ask: "Is occupation a measure for the relief or retardation of certain mental diseases?" "Is it beneficial in all mental diseases?" "If not, in what diseases is it beneficial?" "To what extent should this therapeutic measure be used, and in what stages of the diseases is it indicated or contraindicated?" "Is it harmful to certain patients, irrespective of the type of the mental disease?" These questions, and many others, can not be answered definitely at the present time. They will demand for solution the attention of many workers, using the best scientific methods, before definite answers can be given and before reasonable prescriptions of occupation can be formulated.

The local financial and the broader economic aspects demand attention in a thorough consideration of the subject, but the latter have little bearing upon the welfare of the patients and will not be dealt with here. The narrower financial aspect must be taken into account. Officers in hospitals for the insane must consider the cuts, and the prices and the methods of cooking of beef for bodily welfare and for taste, and in therapy there are similar limitations of expenditures for medicine and medical attendance. The kinds of occupation, the costs of materials and the salaries of teachers or supervisors are important items to be considered if it be definitely established that this method of treatment is to be used in a proper manner and not like the cure-all blood-letting of former days. Assuming the general therapeutic value of occupation to be established, there are a number of questions to be answered: "What kind of occupation is best

fitted to restore certain types of patients to mental health?" "Are the common tasks of the home, of the farm, or of the factory useful means of bringing about cures?" "Or, are those occupations of a nature less familiar to the patient the more beneficial to him?" It will be appreciated that answers to these questions, although of therapeutic importance, have great financial interest. Institutions with considerable endowment need hesitate little, if at all, regarding this aspect of the question, but most state and private institutions must consider it seriously. Whether the patients are best (therapeutically) employed as factory or farm hands, or given a smattering of an art education, or amused, are, however, matters to be determined by scientific investigations. Scientific investigation must also solve the problems of therapeutic potency of various types of occupations.

The results of such investigations will have not only therapeutic and financial interest, but also a purely scientific value. It is to be expected that these inquiries will, for example, help us to appreciate better what dementia is, the similarities of and the differences between the sane and insane, and many other matters in which the psychologist, as well as the psychiatrist, has an interest. On the other hand, the investigation of the course of training, of the possibility of the insane of acquiring habits of a very complex nature, of the curve of forgetting, etc., is directly applicable to the broader problem of occupation. It was largely because of the psychiatric importance of the general problem and the relation to it of the psychological investigation of training that the present work and that of Kent, both from this laboratory, were suggested and undertaken. How far the accumulated facts may be applied to the solution of the psychiatric problem it is too early to predict. At present it is sufficient that the ground has been broken and that by the application of psychological methods there have been accumulated facts which must be considered in a thorough investigation of the important practical problems of occupation therapeutics.

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*October 15, 1912.*

# LEARNING IN DEMENTIA PRECOX

## STATEMENT OF PROBLEM

The present study considers the formation under experimental conditions of certain habits in cases of dementia precox. It aims to investigate the acquisition of motor skill in both simple and complex operations. The writer has regarded his task throughout as a problem in psychology and has endeavored to maintain a consistently psychological point of view. He hopes, however, that a psychological study of material which is more often the subject of psychiatric investigation may prove of interest to both psychologist and psychiatrist,—to the former, because it is a direct contribution to his especial field of knowledge; to the latter, because the psychological facts may well carry a psychiatric implication.

The scientific study of the course of training of the insane has received very little attention. The reports of hospitals for the insane often contain brief accounts of industrial work in which the patients are employed, but the details are too meager to indicate the extent of the work or the classes of patients employed. If accurate accounts of the characters and amounts of work performed have been kept, these have not been made available in scientific publications. With the exception of the study by Kent on *Habit Formation in Dementia Praecox*<sup>1</sup> the writer is aware of no investigation of this problem. His present purpose is to supplement the conclusions of Kent in three respects.

In the first place, the subjects were given a large number of preliminary and supplementary tests, in order that the individual mental characteristics of each subject should be more accurately known. The writer hoped that it might be possible to factor ability in some of these tests into terms of ability in other similar

<sup>1</sup> *Psych. Rev.*, 18, 1911, pp. 375 ff.

tests. That the results of this attempt were largely negative does not, he thinks, make them unworthy of record.

In the second place, a systematic effort was made throughout all the tests of learning to train the subjects to give introspective reports of consciousness during the course of learning. Probably there is no psychologist who does not admit that the fullest account of learning must include, in addition to a record of the performance of the subject, an account of the temporal series of conscious events that occur with that performance. At least half the problem is overlooked when introspections are not required; and, moreover, the interpretations of the behavior of the subject may be wrong. Attention is called elsewhere (pp. 48f.) to a radical difference in the interpretation of data by the writer and the interpretation of similar data by Kent, the difference being due entirely to the availability of additional information furnished by the introspections.<sup>1</sup>

The cause of introspection in work with the insane has already been championed by Ley and Menzerath,<sup>2</sup> who as a matter of fact, however, obtained from their patients not so much actual introspections of conscious experiences as information and statements of meanings. Franz speaks of the introspective ability of some insane and compares them not unfavorably with normal subjects<sup>3</sup> of similar education or experience; but so far as the

<sup>1</sup> It is realized that the reliability of the introspections of persons who are insane may be questioned. For this reason, when space has permitted, there have been printed the introspections upon which the conclusions are based, making it possible for the reader to determine for himself what value he will place upon the reports. The whole question of reliability is too large a one to enter into this article. The writer has, however, recently obtained introspections under the same conditions from uneducated normal observers and from trained introspectors, and has determined by a comparison of these introspections with those of the insane subjects, not only the reliability of the latter, but also the extent to which certain self-made expressions of the subjects may safely be translated into technical psychological terms. (See *Introspection in Dementia Precox*, *Am. Jour. Psychol.*, 24, 1913, pp. 145 ff.

<sup>2</sup> *L'étude expérimentale de l'association des idées dans les maladies mentales*, 1911, p. 32.

<sup>3</sup> *Psychological Opportunity in Psychiatry*, *Jour. Philos., Psych. and Sci. Methods*, 3, 1906, p. 567.

writer is aware no serious attempt has been made to train insane subjects to introspect.

In the third place, the present work was concluded by the study of the learning of a practical industrial operation. It is not always desirable to conclude that the ability to learn in certain standard laboratory experiments necessarily indicates ability to learn a fairly complex operation, such as is met with in everyday experience. For this reason the work of the tests was followed by the learning of an industrial problem, in which the performance of the subjects was measured and quantified, as far as possible, in the same way as in other experiments in learning.

Throughout the work an especial effort was made to avoid the use of complicated apparatus, which may easily disturb insane subjects and interfere with the results. One patient used in these tests (see page 13) would, for example, have resolutely refused to have anything to do with contrivances of an electrical nature, for he would have believed that "electrical influences" would do him great harm. It was in order to avoid the distraction that is so likely to occur with dementia precox patients, that, in two of the tests, the subjects, instead of being blindfolded, were prevented from seeing the work by a small curtain. In all cases the effort was made to restrict the materials employed to the most familiar articles,—paper, pencil, and cardboard. The most complicated piece used was the metronome, which was left in sight for many days before the experiment began and was casually explained to the patient and handled by him at times before it was put into use.

## CASE HISTORIES

The experimental work was performed at the Government Hospital for the Insane, Washington, D. C., in the summer of 1912. Eight subjects were used. These were cases of dementia precox, in some of which the disorders of conduct that gave rise to the diagnoses were of comparatively short duration. There follow the records of these patients, in which will be found general accounts of their histories previous to admission, taken from the records of the hospital, and more detailed ac-

counts of their histories since admission to the hospital, especially during the period of experimentation.

Subject A: (Case No. 19069) 30 years. Resident in hospital 14 months prior to experimentation. Diagnosis: dementia precox, hebephrenic form.

The subject was born of an alcoholic father and a normal mother. One sister is insane. He had great difficulty at school, going only as far as the third grade and failing of promotion several times. At 12 years, he was put to work, and held five positions during the next twelve years. At 24 years he enlisted in the marine corps, where he served five years. After his enlistment, he began to drink to excess and was court-martialed a number of times. He then became subject to periods of depression and four times when depressed attempted suicide by poison. Following his last attempt he was sent to this hospital.

The subject readily became adapted to hospital routine. At intervals he experienced spells of depression, during which he talked very little and was inclined to refuse food. From June 4th to July 10th it was necessary for him to be tube-fed, a process to which he offered violent resistance. Experimental work began June 26th and continued until September 2nd. During the part of this period that he was being tube-fed, the subject cooperated in the tests and answered questions readily, although he spoke very little unless questioned. Later, however, he appeared more interested, and while working at rug-making in August would voluntarily make remarks, although he never conversed freely. In the ward he was normally interested in all that was going on. He did not read, and was most frequently found lying on the seats, sometimes sleeping.

During the preceding two years the subject thought that he was controlled by others, that his thoughts and ideas came from outside sources. He also believed that there had been attempts to do him injury. There were some evidences of auditory hallucinations, but these were not definitely established. During the periods of depression, he became dull and apathetic, was almost inaccessible, and when questioned smiled in a silly manner but gave little information.

Subject B: (Case No. 19949) 36 years. Resident in hospital 15 days before experimentation. Diagnosis: dementia precox, paranoid form.

The subject attended country school between the ages of eight and eighteen, and did well in his classes. He worked in various places until the age of 25, and at one time wandered about as a hobo. He enlisted in the army, where he was twice court-martialed. He was still enlisted at the age of 36, when he gave evidence of the belief that people were annoying him and collecting evidence against him. He was arrested for insubordination and court-martialed. In the guard-house he was found to have perse-

cutory ideas, tactile, auditory, and visual hallucinations. Subsequently he was sent to this hospital.

After admission and during experimentation the subject maintained a constant demeanor. In the ward, he walked around or sat idly and talked at times to himself; he did not read or help with the work and remained by himself almost entirely. He co-operated readily in the performance of the tests, although at first he was prone to interrupt the procedure with a rehearsal of his troubles. He rarely volunteered a remark except about his troubles.

The subject believed that he was held a prisoner at the hospital as the result of a conspiracy of the Roman Catholic priests and the thirty-second degree Masons. These organizations, he believed, were trying to prove him guilty of unnatural practices. Their object in convicting him was to prevent themselves from being exposed by him. He thought that prominent financiers or certain wealthy men were concerned in the affair, and that in some way a number of vessels in the French and Japanese navies were also involved; one of these vessels obtained a crown of diamonds that had been made up for him. His enemies, he said, were doing everything that they could against him, and, wherever he went, were constantly telephoning accusations against him to the people with whom he was. His delusions were very poorly connected and sometimes almost unintelligible, but remained practically unchanged from day to day. He continued to have auditory hallucinations of voices accusing him of immoral practices. He protested constantly that he had been victimized, and that he wished to get out of the institution in order that he might order an investigation.

Subject C: (Case No. 19958) 18 years. Resident in hospital 14 days prior to experimentation. Diagnosis: dementia precox, hebephrenic form.

The subject stated that he had had nervous attacks every summer since he was six years old, and that some of these attacks had kept him in bed as long as three weeks. Between the ages of 6 and 13 he attended school, completing the fourth grade, having failed of promotion twice. At the age of 13, he went to work and continued in various employments until he enlisted in the navy at 17. While on board ship he began to be troubled by the belief that some one was tampering with his mail and that his ship-mates were going to 'initiate' him. On one occasion he jumped down a coal chute, stating afterward that his act was part of the 'initiation'. He was admitted to the hospital about a year after his enlistment.

The subject after admission was quiet, neat and orderly. A great part of the time he was employed in the kitchen, where he worked steadily at whatever he was given to do. He was under experimentation from June 26th to September 1st. On July 24th he was granted a restricted parole. On August 1st he began work on the experimental rug-making. In the early part of July, when not employed, he would sit for long times together, taking no notice of anything about him, unless he was spoken to. Throughout August, however, he was much more alert and active, noticed those

who passed, spoke to the physicians, and sometimes played ball on the grounds. He co-operated readily in all the tests, and tried hard, in a childish manner, to please the experimenter. When making rugs, he talked a little with the other subjects, but in the ward he had very little to do with the other men.

The subject reported hearing voices accusing him of sexual vices and insulting him. He was worried a great deal by these voices. He also worried much over mistakes that he imagined he made in his work in the kitchen. Early in the period of experimentation he stated that he sometimes would hear his thoughts spoken aloud, as if someone had the power to read his mind; also that he perceived 'funny odors', which he could not understand, but which he thought indicated an attempt to 'dope' him. These latter delusions disappeared after the first few weeks of the period of experimentation. He always appeared somewhat confused, but answered questions well and intelligently, if questioned carefully and encouraged.

Subject D: (Case No. 19883) 21 years. Resident in hospital 6 weeks prior to experimentation. Diagnosis: dementia precox, catatonic form.

The subject attended school between the ages of 6 and 16, failing of promotion once, and going as far as the first year in high school. After leaving school, he was employed in a number of clerical positions, from several of which he was discharged on account of incompetence. At this time, he stated, his head and eyes bothered him. Later he secured outdoor work and felt better physically. At the age of 20, he enlisted in the navy. Seven months after enlistment, when on shore leave, he spent a night in drinking and in sexual excesses. Returning to the ship next morning, he attracted attention by jumping overboard. For the next few weeks he was unable to answer questions intelligently, talked constantly about sexual matters, and carried out filthy practices with his excreta. He was transferred to this hospital six days after this sudden change in his condition.

During his residence at the hospital the subject had somewhat improved. He was no longer filthy in his acts, did not remove his clothing, and answered questions, for the most part, intelligently, although only after great hesitation. In the ward, he had little to do with other patients, but sat or lay about stupidly for long periods, or walked rapidly about, often waving his arms. In the experimental work he co-operated in general satisfactorily. Sometimes for periods of over a week, the experimenter was not able to get him to utter a word. The mutism appeared, however, not to be due in any way to lack of comprehension, for he obeyed instructions, even those which were not the simplest, promptly and correctly. Sometimes a period of silence would be broken by a well-formed sentence, containing words not included in his usual meager vocabulary. On one occasion, after a two days' silence, the experimenter was questioning him about his inability to speak, when he suddenly replied, "I think it is the effect of my environment. I can't talk in an institution for the insane." On another occasion, when he had been found reading a book in the ward, he talked very freely. During

the period of rug-making, he was never mute and frequently, contrary to his customary apathetic manner, expressed his pleasure in the work. It was during this period that it was found possible to get introspections for the maze-learning consciousness from him. At all times he was liable suddenly to become unnecessarily precise in all his movements, thus slowing his times of performance in the various tests. These spells interfered with the rug-making as well as with the other tests. He could sometimes be brought again to usual activity by a sharp criticism and an imperative command to do better.

The subject continued to have hallucinations of voices accusing him of vile acts. He frequently laughed in a silly manner, and when questioned replied, "I don't know why I laughed. I just felt as if I had to."

Subject E: (Case No. 19755) 30 years. Resident in hospital 4 months prior to experimentation. Diagnosis: dementia precox, hebephrenic form; alcohol a precipitating factor.

The subject attended school between the ages of 6 and 10 and got along well. After leaving school he worked on a farm until he enlisted in the army at the age of 18. After his enlistment he began to drink heavily. He re-enlisted three times. When 29 years of age, while in service in the Philippines, he stated that he started out on a hunting trip. While lying awake at night, he heard the voice of his commander, saying that he was going to kill him, because of some immoral thoughts that the subject was having. Thereupon the subject started to return and met natives who looked like little black devils. Upon arriving in camp, he thought that the entire troop wanted to kill him, and he got a gun in order to shoot himself. The gun was taken from him. Later, at breakfast, he seized a knife and cut his throat. He was sent to the hospital, where he continued to hear voices and to have tactile hallucinations, involving his legs and genitals. He was returned to the United States and, while being transferred from the transport, jumped overboard. He was immediately rescued, and was transferred to this hospital about eight months after the first appearance of his altered condition.

After admission the subject was active and interested in the ward, playing cards with the other patients and reading a great deal. On one occasion he succeeded in running away and was brought back. He sometimes helped a little with the ward work. At the beginning of experimentation, he incessantly asked for parole and complained about his imprisonment at the hospital. He took little interest in the tests. Later, however, he came to take a keen interest in the experimental work, trying hard to improve his records. He objected at first to the rug-making, but very soon became much interested in that, also. He no longer complained or asked for parole.

During the first part of the period of experimentation, the subject worried very much over voices, which, he said, spoke to him, sometimes accusing him and calling him vile names, and sometimes directing him to take command of the hospital and drill the patients. He would hear these voices in the chirping of the birds and the sound of the trolley cars, and would sit

and repeat to the experimenter what he imagined they said. In the latter half of the period of experimentation, he ceased to talk of the voices, although he stated that they were still present.

Subject F: (Case No. 19465) 22 years. Resident in hospital 9 months prior to experimentation. Diagnosis: dementia precox, hebephrenic form.

A maternal aunt of the subject was insane. He attended school between the ages of 7 and 14 years, going as far as the seventh grade. He was not apt in his studies and was constantly getting into mischief. He worked on a dairy farm for four years, and then, at the age of 18, enlisted in the navy against the wishes of his father, who told him, the subject states, that he would be sure to go to an insane asylum if he went into the navy. After two years of service the patient reports that he began 'acting kind of funny' and that he laughed a great deal of the time. For these reasons he was transferred to the naval hospital. Some months later he was admitted to this institution.

After admission the subject was conspicuous for his desire to work, working steadily all day about the ward or in the dining room. He worked hard and put a great deal of muscular effort into all his movements. At one time, while employed in the dining room, he annoyed one of the female nurses, and for this reason was returned to the ward. Ten days later he attempted suicide by knocking his head against the wall, sustaining a severe laceration of the scalp. He explained that his action was the outcome of his love for the nurse. Experimentation was begun six weeks after his suicidal attempt. He co-operated well in all the tests with increasing interest throughout and with much enthusiasm for the rug-making.

The subject did not report hallucinations, delusions, or other similar mental symptoms. He was dull and slow of thought, whistled and sang in a mechanical manner while working, and sometimes laughed inanely without cause. During the experimental period this laugh, which the subject could never explain, became less and less frequent, finally becoming of rare occurrence. When questioned about his suicidal attempt the subject, early in the experimental period, would become uneasy, flush, perspire profusely, without answering. Later in the period, however, he spoke of it more freely and laughed about it when speaking of it to other patients. He also mentioned other love affairs that he had had.

Subject G: (Case No. 19804) 57 years. Resident in hospital prior to experimentation, 2 months. Diagnosis: probably dementia precox, although alcoholic deterioration has been suggested.

The subject stated that his father was 'queer' and that a brother had had epileptic fits. The subject attended school between the ages of 3 and 13. Thereafter he worked in the dry goods and upholstery businesses until

the age of 24, when he emigrated from Ireland to the United States. He continued as a dry goods salesman in various parts of the country until he was 51, holding a great number of positions in many different cities. He began drinking at the age of 15, and has drunk very heavily most of his life, although he stated that he had never had delirium tremens. His drinking propensities finally made it impossible for him to continue his usual business. For six years he got along on charity and odd jobs. He was arrested in Washington for disturbing the peace by making a violent and blasphemous speech on the street about the dangers of electricity, and was sent to this hospital.

Upon admission the subject appeared pleased with his surroundings and began to work regularly in the dining room. He was granted limited parole on June 14th. Experimental work began on June 26th. On July 2nd the subject became excited and talkative, and went to the Administration Building complaining that he was being illegally held at the hospital. His parole was taken up. After a few days he became quieter and apparently content. Throughout most of the experimental period he co-operated in the experiments, showing great interest in the tests. On August 1st he began to make rugs. He complained, however, that this was 'woman's work', that he was illegally detained at the hospital, and that he could not be forced to work. He was persuaded to continue until August 7th, when he refused to make rugs or to fill in the cancellation forms, saying that the latter hurt his eyes. He continued to work with the maze, but remained irritable and no longer appeared to take interest in the work.

The subject stated that he 'heard voices' about twenty years ago and that he began to be affected injuriously by electricity ten years ago. He stated that the latter trouble was due to an electric plate in his brain, which had telephonic connection with electric influences outside. He made a great many irrelevant references to electricity in the two excited spells above mentioned. He sometimes talked to himself, but denied that he still heard voices. His memory was excellent, and his general information good. He talked constantly and expressed himself very well for one of his education.

Subject H: (Case No. 19982.) 31 years. Resident in the hospital one week prior to experimentation. Diagnosis: dementia precox, hebephrenic form.

One brother of the patient was neurotic, one paternal grandaunt, insane. The subject received a high school education in Roumania, came to the United States at the age of 19, and worked steadily at photography for the next few years. At 24 he enlisted in the army, where he had severe bronchitis and later a hemorrhage from the lungs. Thereafter he became depressed, worried about his condition, and had auditory and visual hallucinations. He was admitted to this hospital at the age of 28, and was discharged as sufficiently improved seven months later. He went to a soldier's home, but continued to have auditory hallucinations, and for this reason was returned to the hospital 18 months later.

Immediately after his admission the subject worried a great deal about his belongings, which had not been sent with him. He became a little more cheerful later. He was given a limited parole, which was taken up August 6th because he violated the limitations. Experimental work lasted from June 26th to August 22nd. During this time he co-operated in all the tests, although at the beginning of some he refused to do so, until the purpose of the test was partially explained to him. His attention was, however, prone to wander to his worries, and his complaints during the experimental hours were so numerous, that it was decided not to put him at rug-making. Later, however, he asked that he might be allowed to try to make a rug, and he was given the frame of Subject G, when the latter refused to work. On August 22nd, however, he suddenly complained that he was being forced to work, and positively refused either to continue with the rug or to perform the tests thereafter.

Besides the worries mentioned above, the subject was disturbed by auditory hallucinations of voices calling him vile names and sometimes saying unintelligible things. He carried a pad and took notes of incidents that happened during the day, representing the unintelligible voices by geometric designs. He had also very mild persecutory ideas, in that he believed that the other patients tried to hurt him or annoy him.

## EXPERIMENTAL PLAN

For all work, except rug-making, the subjects reported for an hour every other day; the interval between the series was, thus, always (approximately) 48 hours. The following tests were performed:

### *I. Preliminary Tests*

1. *General tests.* For the first two weeks, in order to accustom the subjects to the type of work and to acquaint the experimenter with the individual subjects, tests of a general nature were performed. The results for three of these,—attention, memory span, and apperception,—all of which were performed upon all subjects under the same conditions, will be briefly stated, as it is thought that they indicate to some extent individual differences of the subjects.

2. *Directions test.* It was thought that a measure of the ability of the subjects to understand instructions would be desirable at the outset.

### *II. Tests of Motor Control*

1. *Tapping test,* designed to measure the greatest speed at which a simple muscular movement can be performed.

2. *Aiming test*, designed to measure the accuracy of a simple muscular movement and to determine its dependence upon the speed of the movement.

### III. Tests of Learning

1. *Kinesthetic memory test*, designed to indicate the accuracy of kinesthetic memory of a simple movement.

2. *Cancellation tests*, designed to test the acquisition of skill in a complicated operation, and the effect of skill acquired in one operation upon the acquisition of skill in another very similar operation.

3. *Maze tests*, designed to measure the acquisition of skill with and without visual perceptual cues, and to determine as far as possible the part played in the learning by kinesthetic memory.

### IV. Learning of a Practical Problem

The subjects were taught to make rugs; their work was quantified and measured as far as possible, for purposes of comparison with the usual laboratory tests of learning already applied.

It will be observed that the tests named above fall into two classes,—tests of motor control, which are pure behaviour tests, and tests of learning, which are more strictly psychological, since they consider, not only the actual performance of the subject, but also the conscious processes involved in learning. It seemed dangerous, when consciousness was to be considered, to rely entirely upon the method of comparative psychology and to infer in the subject the consciousness which we find to be typical in normal subjects. Accordingly, as has already been mentioned (p. 6), an attempt was made, wherever possible, to secure introspections from the subjects.

## I. PRELIMINARY TESTS

### I. GENERAL TESTS

a. *Attention with simple stimuli*. A series of 150 digits<sup>1</sup> was read at the rate of two per sec. to the subject, who was instructed

<sup>1</sup> The series used was the first one given in Franz: *Handbook of Mental Examination Methods, Nervous and Mental Disease Monograph Series No. 10, 1912, p. 71.*

to tap on the table with a ruler whenever the digit "3" occurred. This digit occurred 25 times. One error was scored whenever the subject failed to tap for "3" or when he tapped for some other digit. When the reaction was obviously delayed, so that the subject failed to tap for "3" but tapped for the next number, only one error was counted instead of two. The test was repeated three times. The average error, as a percentage of the total 150 digits wrongly responded to, is shown for each one of the eight subjects in Table I.

TABLE I  
ATTENTION TO SIMPLE STIMULI

Subject	A	B	C	D	E	F	G	H
Per cent. error.....	.9	2.7	2.2	16.5	3.1	2.7	2.2	6.4
Rank of subject.....	1	4.5	2.5	8	6	4.5	2.5	7

*b. Memory span.* The immediate memory span for a series of digits was tested with both auditory and visual stimuli. For the auditory presentation the numbers were read to the subject at the rate of two per sec.; for the visual presentation black gummed letters on a light gray cardboard slide were presented one after the other through a window in a cardboard slide holder<sup>1</sup> at the rate of 2 per sec. The subject was, in each case, first presented with two series of three digits each, then two series of four digits each, then two series of five digits each, and so on up to ten digits. The results are shown in Table II. The memory span was taken as the greatest number of digits correctly repeated, although the first mistake may have occurred for a smaller number. The combined ranking of the subjects for the two series is based on the average span for each subject.

TABLE II  
IMMEDIATE MEMORY SPAN

Figures indicate the greatest number of digits correctly reproduced. Stimuli presented serially.

Subject	A	B	C	D	E	F	G	H
Auditory Stimulus.....	7	7	7	5	7	7	7	8
Visual Stimulus.....	5	5	5	3	5	6	5	5
Rank of Subject.....	5	5	5	8	5	1.5	5	1.5

<sup>1</sup> The apparatus was very similar to that described by Franz, *ibid.*, p. 95 f.

*c. Apperception tests.* The Heilbronner test was used.<sup>1</sup> In this test the subject is presented successively with a series of cards with drawings upon them. The first card has in bare outline the principal parts of an object, the next is slightly more complete, the next still more so, and so on to the last card, which bears the object completed with enough detail ordinarily to insure recognition. Series were used with pictures of the following objects: bicycle, fire-place, fountain-pen, lamp, phonograph, telephone, watch, and windmill.<sup>2</sup> The subject was asked to state what he thought each picture represented or, if he could not do that, to describe what he saw. The results appear in Table III. The degree of apperception for each series is expressed somewhat arbitrarily as the percentage of the cards in the series recognized correctly by the subject. In only one or two instances did these cards fail to be the final consecutive ones for the series. The figures given in the table are the averages of the percentages obtained in this way.

TABLE III

## HEILBRONNER APPERCEPTION TEST

Average of *per cents.* of total drawings presented, that were correctly identified in each series of cards.

Subject	A	B	C	D	E	F	G	H
Per cent. identified.....	69.6	42.1	60.9	40.5	58.8	61.1	65.1	82.6
Rank of subject.....	2	7	5	8	6	4	3	1

## 2. DIRECTIONS TEST

In order to obtain some insight into the ability of the subjects to understand and to act upon simple instructions, they were given the standard "directions tests" prepared by Woodworth and Wells.<sup>3</sup> Both the easy and hard tests were given, the two halves of the easy test being given on different days. All the subjects could read and write, although some hesitated slightly and tended to misread when they did not understand the question. They were told to "do everything that it tells you to do on the

<sup>1</sup> The Ebbinghaus completion test, as described by Franz, *ibid.*, pp. 77 f, proved too difficult for these subjects.

<sup>2</sup> The series are illustrated in Franz, *ibid.*, pp. 80-82.

<sup>3</sup> *Association Tests, Psych. Rev. Monograph*, No. 57, 1911, pp. 68 ff.

sheet." They did not know that they were to be timed. Table IV shows the time per reaction (each sheet is supposed to involve 20 separate reactions), and the percentage of instructions incorrectly responded to. With normal subjects the tests should be performed without error, the significant value being the time per reaction. For the precox patients, however, the tests were so difficult that they were in no case done perfectly and the percentages of error were often very high, in spite of the fact that the instructions had not been to work as fast as possible. The ranks given for the subjects in the table are thus based not on the reaction times, which are of minor significance, but upon the percentage of error.

TABLE IV

## DIRECTIONS TESTS

For the understanding of instructions. Figures show time in secs. per reaction to instructions and *per cent.* error of the reactions.

Subject	A	B	C	D	E	F	G	H
Easy tests:								
Secs. per reaction....	15.1	14.6	29.7	14.1	10.2	25.5	16.7	14.8
Per cent. error.....	10.0	17.5	22.5	50.0	22.5	15.0	12.5	2.5
Hard tests:								
Secs. per reaction.....	17.2	19.6	51.0	12.0	12.4	18.5	12.0	15.1
Per cent. error.....	10.	20.	65.	65.	15.	25.	50.	40.
Rank of subject.....	1	3.5	7	8	5	3.5	6	2

## II. TESTS OF MOTOR CONTROL

## I. TAPPING TEST

Upon ordinary ruled paper the subjects were required to make dots with a pencil, back and forth across the paper, as rapidly as possible for 30 seconds. The test was performed by each subject on three different days. The average number of dots per second with the mean variation for the three performances is given in Table V. Gatewood, who used this same test with dementia precox patients, calls it a test of "motor efficiency", but observes that a high speed may not be a measure of "motor efficiency" as the performance may be "automatic and not the result of voluntary control".<sup>1</sup> Just when ease of forming

<sup>1</sup> *An Experimental Study of Dementia Praecox*, Psych. Rev. Monograph, No. 45, 1909, p. 63.

automatisms becomes a mark of inefficiency rather than efficiency for the human organism it is not our present purpose to determine. Certain it is that the establishment of an automatic movement, necessary in the performance of such mechanical operations as those with which we are concerned in this paper, is economical and advantageous for the rapid acquisition of skill. Accordingly the subjects have been ranked for speed attained.

TABLE V

TAPPING TEST

Figures show average numbers of dots made per sec. and mean variations for three 30-sec. tests.

Subject	A	B	C	D	E	F	G	H
Dots per sec.....	6.0	4.5	6.0	3.1	6.8	5.4	6.0	6.4
M. V. ....	$\pm .13$	$\pm .68$	$\pm .22$	$\pm .69$	$\pm .30$	$\pm .32$	$\pm .18$	$\pm .46$
Rank of subject.....	4	7	4	8	1	6	4	2

The average speed for the eight subjects is  $5.5 \pm .86$  dots per sec. It is interesting to note that this speed is a little greater than that obtained by Gatewood for dementia precox patients, much nearer in fact to the rate found by him for normal subjects. Computed approximately from his curves,<sup>1</sup>—he does not give the data,—the average for five precox subjects is  $4.3 \pm 1.12$  dots per sec.; for four normal subjects,  $5.9 \pm .6$  dots per sec. It is probable, however, that deterioration has progressed farther in Gatewood's patients than in those of the present study. Franz finds for two normal subjects an average of 6.44 dots per sec.<sup>2</sup>

## 2. AIMING TEST

This test, it will be recalled, was designed, primarily, to measure the accuracy of a simple muscular movement, and, secondarily, to determine the dependence of the accuracy upon the speed of movement.

Printed forms were prepared with five circles (18-point capital O's were used) arranged as shown reduced in Fig. 1. The circles were 5 mm. in diameter and were so placed that the

<sup>1</sup> *Ibid.*, p. 65.

<sup>2</sup> *Time of Some Mental Processes in the Retardation and Excitement of Insanity*, *Amer. Jour. Psychol.*, 17, 1906, p. 17.

distance between centers from "1" to "2" should be 50 mm.; from "2" to "3", 75 mm.; from "3" to "4", 100 mm.; from "4" to "5", 50 mm.; and from "5" to "1", 100 mm. The subject was required to take a pencil in the right hand, and with a movement of the whole forearm to make dots, in time with a metronome, successively upon the five circles in the order numbered, beginning over again after marking "5". The exact manner of holding the pencil was left for him to determine.

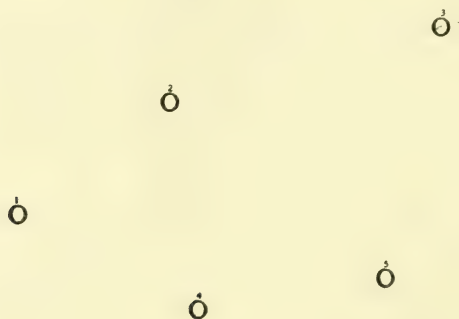


Fig. 1. Target, used in Aiming Test.

Three speeds were used,—1 per sec., 2 per sec., and 3 per sec. The subject made 100 dots on each circle (*i.e.*, 500 dots in all) at each speed on each of six days. Twenty-five dots on each circle were made on each sheet, that is to say, the subject completed the circuit of the five dots 25 times on each sheet. Thus four sheets at each speed, 12 sheets in all, were made each day. In order to balance the effects of fatigue and practice as much as possible, the order of the three speeds was varied from day to day, so that each speed was used an equal number of times in all positions of the series, and was preceded and followed an equal number of times by each of the other speeds.

Before beginning the series, the subjects were practised in keeping time with the metronome at the three speeds, so that they might not find it too difficult when the test was begun. As it was, three subjects found the form used so much harder than the practice form, that they were unable to maintain the highest speed on the first day of the series.

The method employed for scoring the errors was as follows: Any dot lying inside the circle of 5 mm. diameter was counted as perfect. Thus dots on the line were counted perfect. Dots outside the circle, wholly or in part, but less than 5 mm. away counted one error; dots between 5 mm. and 10 mm. from the circle counted 2 errors; dots between 10 mm. and 15 mm., 3 errors; and so on. Thus the errors given represent, not the number of dots outside of the circle, but a moment<sup>1</sup> of error of these dots about the circle.

The method of scoring implies that, when all the dots outside of the circle have been counted, enough more to make 25 must lie within the circle or upon the line of the circle. As a matter of fact careful examination did not always reveal twenty-five distinct points. Sometimes there were as few as eighteen. This was due apparently to the fact that some dots coincided. As long as the coincidence occurs within the circle, no error is introduced; but if the coincidence occurs outside the circle the score is, of course, rendered slightly smaller than it should be. The defect seems to be inherent in the method, as examination with a low power microscope giving a magnification of 25 diameters, showed that two points were always distinguished as separate unless the coincidence was so exact as not to be observable under magnification. It is thought therefore that the error in scoring introduced in this manner is very small. The dots within the circle were nearly always more numerous,—generally very much more numerous,—than those lying within the imaginary ring in which the error was scored one, and since the area of the circle is one-eighth the area of the ring, the chances are very much greater that the coincidence occurred within the circle. Moreover, the likelihood of a coincidence occurring in the area immediately outside the circle is only great when the number of dots falling within it is large; but when this number is large, as it is at a high rate of speed, there are also many dots farther removed from the circle. The total score in such case is apt to be about forty. Very roughly speaking, we would not expect in such a case to find more than six coincidences, four lying within the circle, two immediately outside and none beyond,

<sup>1</sup> The error values are not, strictly speaking, moments about a point. They are the product of the number of dots by their average distance, not from a point, but from a 2.5 mm. circle, expressed in 5 mm. units. It was thought, however, that the term "moment" expressed the significance of the scores better than any other word, and it is used with this explanation.

—for the distribution beyond is always very sparse. The error introduced, then, by coincidence of points would not be greater than 5 per cent. We can perhaps claim no greater accuracy for the method than this. In a similar test Woodworth<sup>1</sup> allowed fifty dots about a single point and counted the outer sixteen. The writer allowed 25 dots about a point and seldom counted as many as sixteen.

The form of the test and method of scoring used were chosen because it was desired, not to make a careful analytical study of the relation of speed to accuracy, but to test in the gross the ability of certain subjects to perform a simple mechanical operation, such as is involved in many skilled occupations, with a certain degree of precision. In any ordinary skilled movement there are limits within which the movement is for all practical purposes perfectly performed. To approximate these conditions, circles were used instead of the points employed by Woodworth<sup>2</sup> and any dot falling within the circle was counted perfect, making an errorless performance quite possible, instead of infinitely improbable. For the same reason, no effort was made to factor the error into a constant error and a variable error. The moment of error used expresses merely the extent of the failure to do the thing required. Again, the circles were arranged asymmetrically, instead of in the equilateral triangle which Woodworth adopted, in order to approximate more closely the irregularity of the componentets of most ordinary movements.<sup>3</sup>

In presenting the results, both in general and for the individual subjects, we shall consider (a) the effect of practice upon accuracy, (b) the effect of the distance moved through upon accuracy, and (c) the effect of the rate of movement upon accuracy.

*a. Effect of practice upon accuracy.* Each subject made 1500 dots at each of six successive sittings, forty-eight hours apart. It might be expected that the subjects would improve in accuracy with this amount of practice. The variations from day to day

<sup>1</sup> *The Accuracy of Voluntary Movement, Psych. Rev. Monograph, No. 13, 1899, p. 23.*

<sup>2</sup> *Loc cit.*

<sup>3</sup> For the suggestion, that the conditions of the ordinary mechanical operation be more closely approximated by the substitution of an asymmetrical arrangement of circles for a symmetrical arrangement of dots, the writer is indebted to Dr. Franz.

are, however, very erratic for all the observers. If anything, there is a tendency for the error to increase slightly after the first day, due possibly to a partial lapse of the *Aufgabe*, which was evidenced also by an increased tendency on the part of some of the subjects to make remarks while working. They were of course constantly encouraged to do their best, but they found the work tedious and continued attention difficult.

Table VI gives the average error per 100 dots made for all eight subjects. The figures for the individual subjects are not given because they fluctuate irregularly and without apparent significance.

TABLE VI

AIMING TEST

Figures show average error per 100 points for eight subjects at six successive sessions, 48 hours apart. There is no increase of accuracy with practice.

Day	I	II	III	IV	V	VI
Rate: 1 per sec.....	9.7	8.7	10.9	7.8	8.3	8.8
Rate: 2 per sec.....	28.6	36.7	39.1	33.4	34.2	38.7
Rate: 3 per sec.....	82.1	112.2	103.2	88.2	87.5	94.9
Average for 3 rates.....	40.2	51.2	51.2	43.1	43.8	47.5

It thus appears that there is no improvement in accuracy with continued practice under the conditions of this experiment. The failure to improve may be the result of the extreme simplicity of the movement made. It is possible that, in comparatively short times at least, no measurable improvement in accuracy can be made in such a simple movement as the making of a dot upon a certain place upon a piece of paper. Improvement does occur in a more complicated feature of the experiment. Occasionally the subjects made gross errors, such as striking the circles in the wrong order or leaving out a circle. The total numbers of errors of this sort made by all subjects is shown in Table VII. For such a complicated movement as following the irregular order of circles on the paper there is marked improvement with practice.

TABLE VII

## AIMING TEST

Figures show total number of gross errors (missing a circle or striking a circle in the wrong order) made by 8 subjects at 6 successive sessions, 48 hours apart.

Day	I	II	III	IV	V	VI
Rate: 1 per sec.....	14	1	0	1	4	2
Rate: 2 per sec.....	1	9	2	1	1	1
Rate: 3 per sec.....	28	14	6	2	0	0
Total for 3 rates.....	43	24	8	4	5	3

*b. Effect of extent of movement upon accuracy.* In passing about the circuit of the five circles the subject is required to move twice through a distance of 100 mm., once through 75. mm, and twice through 50 mm. It was thought worth while to examine the data in order to determine whether the distance moved through before making a dot had any effect upon the accuracy with which the dot was placed.

Table VIII shows the average error per 100 dots for each of the five circles, calculated for all eight subjects on all six days. The same result is shown graphically in Fig. 2, where the diameter of each large circle is proportional to the average moment of error at that point. The absolute size of the circles is without significance.

TABLE VIII

## AIMING TEST

Figures show moments of error per 100 dots and are averages for 8 subjects upon six days; also *per cent.* correlation of distance moved through before point with error at point.

No. of circle	I	2	3	4	5	% cor. for 1, 2, 3, 4, 5,	% cor. for 2, 3, 4, 5,
Rate: 1 per sec.....	8.2	8.4	9.9	9.6	9.1	10.3	69.7
Rate: 2 per sec.....	35.3	28.8	39.2	37.0	35.3	50.7	63.7
Rate: 3 per sec.....	66.9	78.9	106.4	111.4	91.5	10.1	89.1
Av. for three rates...	36.8	38.7	51.8	52.7	45.3	23.7	74.2

Examination of the figure would seem to indicate that, in general, the greater the distance moved through the greater the moment of error in making the dot at the point moved to; but that an exception occurs in the case of circle No. 1, where the error is least and the distance maximal. These facts are shown quantitatively in the coefficients of correlation (Pearson method of

'productive-moments') of distance moved through and subsequent error, also given in the table. When all five circles are considered, the average coefficient is 23%, but if circle No. 1 be excluded, the average coefficient for the other four is 74%.

The introspections of the subjects seem to indicate that the conditions for circle No. 1 are not the same as for the other circles. The subjects make such reports as these: "I do the same thing over and over again." "It [the metronome] plays a sort of tune; I'd know it was five without counting." "It's three and then two to me, and then I begin over again." "It's

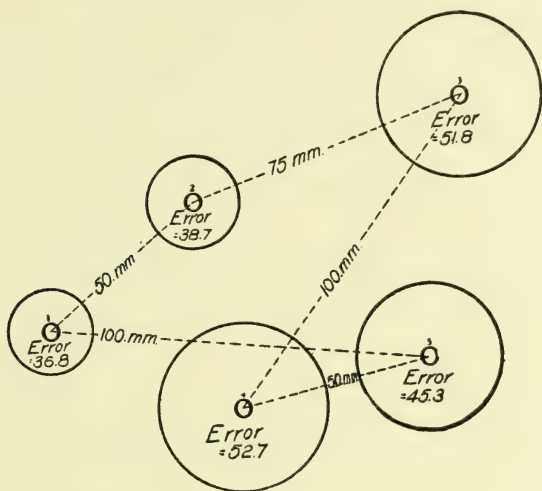


Fig. 2. Errors in Aiming Test. Diameters of circles are proportional to average of 14,500 moments of error for each point. Figures indicate the actual values of the average moments and the distances between points. Note that distance moved through is correlated with size of subsequent error except for 1.

harder to get them good down here [circles No. 4 and No. 5]; I can do it better up here at the start [circles No. 1 and No. 2].” In other words, it appears that the subject conceives that he is beginning over again when he gets to circle No. 1. He does not make 125 consecutive dots, but 25 rythmical units of five dots each, beginning always at No. 1. Sometimes, though not always, it was possible to note a distinct pause before striking at circle No. 1. The writer, in performing the test, found a tendency toward a slight hesitation before striking No. 1 which came as the initial member of a three-two rhythm, toward little better fixation, and, in general, for the whole movement process

to be presented more clearly in consciousness. For these reasons, it seems fair to assume that different conditions apply to the initial point and to exclude it when computing correlations. Thus we find that there is a high positive correlation between distance moved through and subsequent inaccuracy, a relation already much more thoroughly worked out by Woodworth<sup>1</sup>, who found that the increase in error was proportional to a value lying between the distance and the square root of the distance moved through, a course intermediate between that which might be expected from Weber's law and that which would accord with the formula of Cattell and Fullerton.

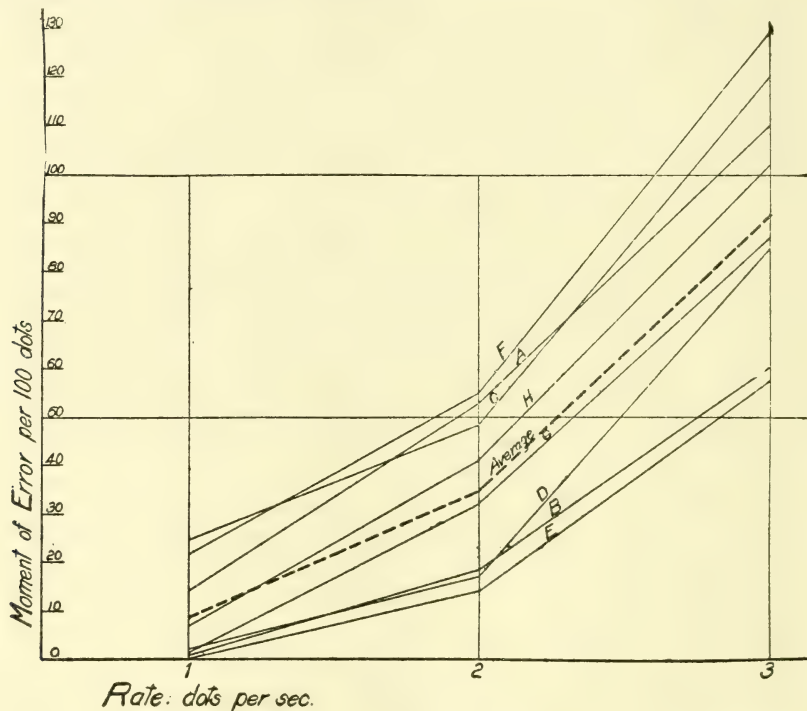


Fig. 3. Relation of speed to accuracy in Aiming Test. Average curve and individual curves for 8 subjects. Three points on each curve.

*c. Effect of rate of movement upon accuracy.* The relation of accuracy to the speed of movement is shown in Table IX. The figures for each observer are moments of error per 100 dots and are averages of 3000 dots made upon six different days. The averages for the eight subjects also appear. In Fig. 3 these

<sup>1</sup> *Ibid.*, pp. 64 f.

values are platted in curves. It is sufficiently obvious that the error increases with the rate of movement. Just exactly what law the speed-accuracy relation obeys is too difficult a question for us to consider with such meager data as a basis.<sup>1</sup>

TABLE IX

## AIMING TEST

Relation of rate of movement to accuracy. Figures show moments of error per 100 dots for each subject, and are averages of 3000 dots made upon six different days.

Subject	A	B	C	D	E	F	G	H	Av. for 8 subjects
Rate: 1 per sec....	72.3	5.4	126.1	10.2	.2	110.9	7.0	34.8	45.2
Rate: 2 per sec....	266.0	94.4	243.4	87.3	71.3	277.4	161.5	208.5	175.6
Rate: 3 per sec....	554.1	304.1	606.3	429.1	292.8	652.8	438.5	513.4	455.1

## III. TESTS OF LEARNING

## I. KINESTHETIC MEMORY TEST

The following test was devised in order to test in as simple a manner as possible, kinesthetic memory, which may be a factor upon which the learning and automatization of a movement depend.

<sup>1</sup>The curves of Fig. 3 have been drawn straight without reference to any theoretical form. One subject (B), who had been doing very consistent work with the test, was tested at eleven different speeds, from 1 to 3.5 per sec. Five hundred dots, distributed over four days, were made at each speed. With one exception they fall along a smooth curve, as is shown in

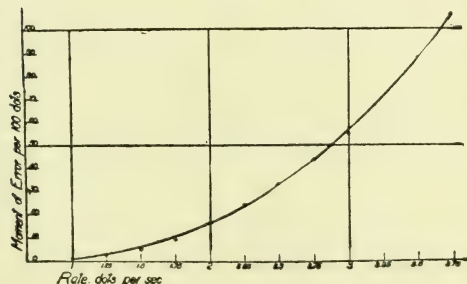


Fig. 4. Speed-accuracy curve for one subject (B) in Aiming Test. 11 points are shown. Curve drawn is that in which increase of error is proportional to square of the rate.

Fig. 4, where the actual curve drawn is that for which the increase of error with the speed is proportional to the square of the speed. It does not follow the direct proportion that would be in accordance with Weber's law nor the square-root-of-the-magnitude relation proposed by Fullerton and Cattell. Cf. *On the Perception of Small Differences* by these

authors, 1892, pp. 152 ff. The curve shown here agrees with that found in some cases for the left hand by Woodworth, *op. cit.*, pp. 29 ff. Of course the data are too few for any generalization.

Strips of cardboard were prepared, 250 by 45 mm., and about 2 mm. thick. Beginning near the left end, two parallel horizontal slots were cut, 15 mm. apart, and each 3 mm. wide. On four separate slips the upper slots of the pair were cut 20, 40, 80, and 160 mm. long respectively, and the lower slots, 200, 200, 200, and 240 mm. respectively. A fifth strip with slots 100 and 200 mm. was prepared as a trial card, which was shown first to the subject and upon which he was practised in order that he might become familiar with the method. It was not used thereafter. The procedure avoids the use of any disturbing complicated apparatus. The method is somewhat similar to that of Münsterberg, and of Fullerton and Cattell in similar work.

The subject was seated at the end of a table and a curtain was suspended before his face, extending from the height of the top of his head to the top of the table. The subject could thus place his hand beneath the curtain and write upon the table without being able to see his hand or even any part of the table. To the unaccustomed observer, especially to the easily disturbed dementia precox patient, blindfolding, if tolerated at all, acts as a distraction. The use of the curtain for obscuring the work lacked this disadvantage and proved satisfactory in all other respects.

The cards were laid upon the table near the curtain but on the side away from the subject. The subject's hand, holding a pencil, was then placed so that the pencil point was at the left-hand end of the upper and shorter slot. The instruction was given, "Move as far as you can." As soon as the subject had completed the movement his hand was placed at the left end of the lower and longer slot, with instruction, "Move the same distance." Each card was presented ten times in succession on each of four different days. Twice the shortest distance was presented first and the longest last with the intermediate distances between; and twice the opposite order was used.

In Table X. there appear for each length (1) the constant error, *i.e.*, the amount that the average reproduced length is

greater or less than the stimulus length, (2) the mean variation from the average reproduced length, and (3) the average error of the reproduced length from the stimulus length. For the average of the four lengths the constant and average errors are given in percentages of the stimulus length. The rank of the subject was based upon the average error, since that error involves both the constant and the variable error.

TABLE X

## KINESTHETIC MEMORY TEST

Figures show in mm., for 4 stimulus lengths, constant error, mean variation, and average error of reproduced lines, and are averages of 40 trials distributed over four days. Averages of the 4 stimuli are given as a percentage of the stimulus.

Subject	A	B	C	D	E	F	G	H	Av. for 8 subjects
Stimulus: 20 mm.									
Constant error...	8.	5.	15.	4.	3.	2.	8.	8.	6.6
M.V.....	3.0	2.4	8.7	6.3	3.2	1.7	2.8	3.6	4.0
Av. error.....	8.4	4.6	15.1	6.9	3.8	2.5	8.0	8.0	7.2
Stimulus: 40 mm.									
Constant error...	14.	8.	6.	3.	6.	12.	7.	0.	7.0
M. V. ....	2.9	3.4	3.7	3.7	3.5	5.0	3.3	2.6	3.5
Av. error.....	14.5	8.2	7.3	4.3	7.5	11.9	7.9	2.6	8.0
Stimulus: 80 mm.									
Constant error...	16.	19.	2.	3.	4.	12.	14.	12.	9.0
M. V. ....	3.8	5.6	6.6	7.9	5.4	11.8	7.3	7.0	6.9
Av. error.....	16.4	18.8	6.4	7.9	6.4	13.8	14.5	13.8	12.2
Stimulus: 160 mm.									
Constant error...	19.	25.	6.	8.	3.	0.	34.	0.	9.8
M. V. ....	11.0	13.0	12.2	21.0	9.0	10.4	12.2	7.2	12.0
Av. error.....	19.2	25.5	13.9	21.4	10.0	10.4	34.4	7.2	17.7
Av. for 4 stimuli.									
% const. error...	27.	20.	22.	4.	9.	14.	24.	14.	
% av. error.....	28.	20.	28.	17.	13.	16.	25.	17.	
Rank of subject....	7.5	5	7.5	3.5	1	2	6	3.5	

It will be observed in the table that all the observers showed a tendency to overestimate the distance, the average overestimation for the eight subjects varying from 4% to 27%. For the four different lengths this constant error increased with the length from 6.6 to 9.8 mm. overestimation. Expressed in percentage of stimulus, however, the constant error decreases

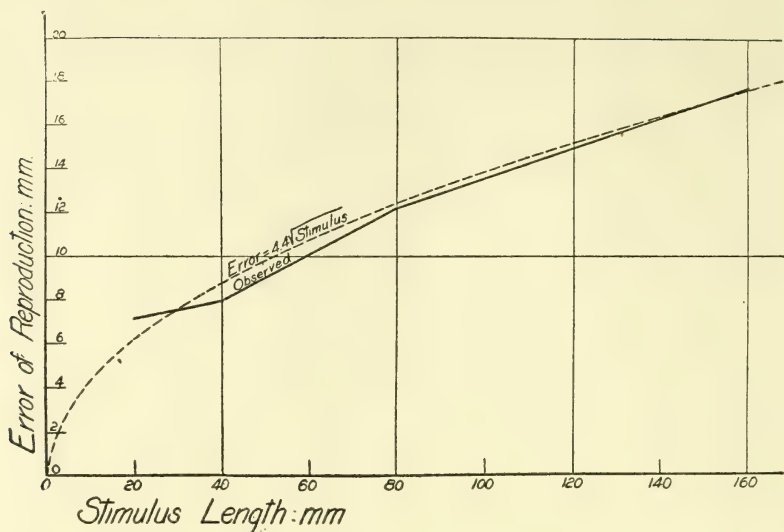


Fig. 5. Kinesthetic Memory. Relation of error of reproduction to stimulus length. Four points. Observed values shown by full curve; theoretical values, in which the error is proportional to the square root of the stimulus magnitude, shown dotted.

from 33% to 6%, as the stimulus increases from 20 to 160 mm. It is possibly caused by the fact that the stimulus movement is checked suddenly and forcibly by the end of the slot. An equal amount of energy expended in the lower slot would carry the pencil further. The absolute values of the mean variations increase with the stimulus length more rapidly, and for the longest length in some cases become actually larger than the constant error. The average error, which depends upon both of these errors, must therefore increase with the stimulus length. The relation of the average values of the average error to the stimulus length is shown in the curve of Fig. 5. Here the four observed points are shown, closely approximating the theoretical relation, in which the error varies as the square root of the magnitude of the stimulus.<sup>1</sup>

## 2. CANCELLATION TESTS

It was the object of this experiment to test the acquisition of

<sup>1</sup> Cf. Fullerton and Cattell, *op. cit.*, pp. 47 ff.

skill in a fairly complicated operation, to determine the character of the operation when learned and the conscious terms in which it was carried, and to study the transfer of practice from the first operation learned to a second operation, in which the motor adjustments remained unchanged, while the perceptual cues were altered.

*a. Procedure.* Three cancellation forms were used, P and Q as shown in reduced size in Figs. 6 and 7, and a third composed of single digits.<sup>1</sup> All three forms were identical in arrangement, containing eight different characters, each occurring twenty-five times, and placed in the same positions on all the sheets. The

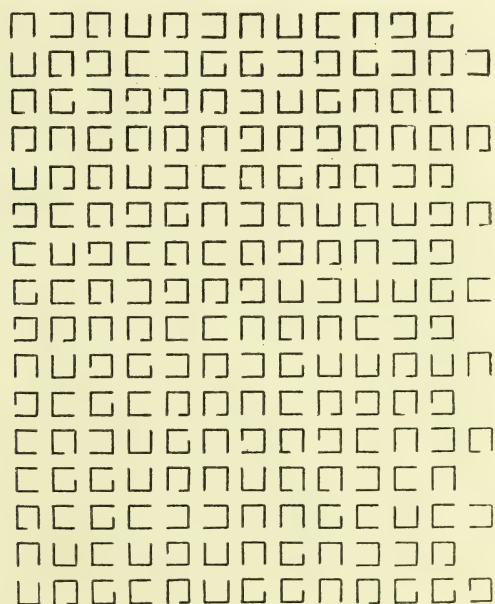


Fig. 6. Form P.; Cancellation Test. Third character from left on top line was cancelled.

symbol selected for cancellation was the third from the left on the top line in each of the two forms shown in the figures. In the digit form the "3", corresponding in arrangement with the second symbol from the left on the top line of the forms

<sup>1</sup>These forms were the same as those used by Kent, *op. cit.*, and are shown by Franz, *Handbook*, pp. 129-132.

shown, was used. Each subject worked, without interruption, on alternate days throughout all the series. On each day he cancelled successively five forms. He used a soft pencil. A copy of the figure to be cancelled was placed before him on a card on a little stand. Errors, both of omission and commission, were marked prominently in blue pencil on each form as soon as it was completed, and the form thus marked was shown to the subject for approximately three seconds, before the subject proceeded to the next.

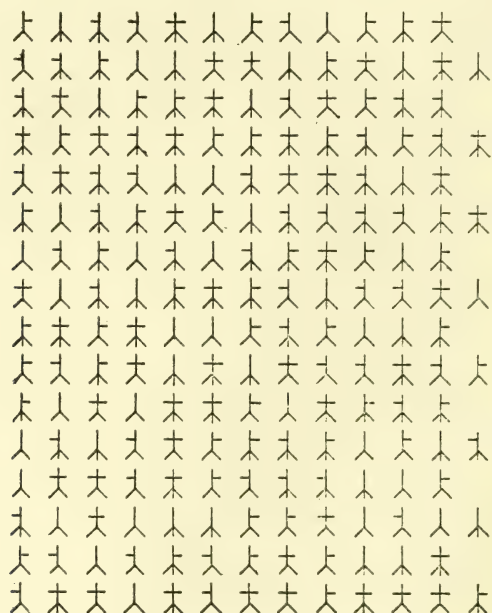


Fig. 7. Form Q; Cancellation Test. Third character from left on top line was cancelled.

The subject knew that the time of his performance was being recorded, and was sometimes told when he had decreased his time. The effect of informing him of his attainment in both accuracy and speed was to establish a double *Aufgabe* to work both accurately and rapidly.<sup>1</sup>

<sup>1</sup> It will be observed that the *Aufgaben* were not the same for all subjects, as some tended to lay greater stress upon improvement in accuracy, others upon improvement in speed, while others held to an intermediate course,—

The digit form was used first in a practice series, which lasted for four days, making twenty sheets in all. It was the object of this series to familiarize the subject with the kind of operation involved—the making of strokes upon a piece of paper in accordance with a visual perceptual cue—so that increased familiarity with the situation would not enter as a considerable factor in learning with the forms following. After the preliminary digit form, four subjects were given form P and four form Q. It was intended to keep all the subjects on these forms until all had become perfect in their performance. Errors, however, persisted with some subjects for such a long time, that it was found necessary to terminate the series for all subjects on the twelfth serial day, when the performance was nearly but not absolutely perfect. Following this series, the subjects were given the form that they had not already used, Q or P, as the case might be. Here they cancelled a different symbol, arranged, however, in exactly the same manner as the symbol they had cancelled in the preceding form. It was desired to see whether the training received with the first form would prove advantageous in learning the second.

*b. Learning.* Learning of this kind of operation was first evidenced in the preliminary series, the results of which are given in Table XI. The figures given are the averages for the five sheets cancelled on each day. The figures show an average

at least such is the indication of the practice curves; the subjects did not specifically describe the *Aufgaben* under which they were working. For F apparently the *Aufgabe* was changed between series I and II, a result of special instructions given him (p. 35).

It is possible that a more definite emphasis in the instructions of the importance of either accuracy or speed might have brought results from the test, that could have been more easily compared than these actually obtained. The necessity of grading the test for two independent variables and the fact that some subjects improved mostly in accuracy and others mostly in speed made comparison difficult and in some cases impossible (see p. 47). Had the importance of either variable been emphasized to the partial exclusion of the other, it is possible that the factor emphasized might become nearly constant and that the variability of the other factor might better serve as a comparative measure of learning. The writer is inclined to believe that in a repetition of the work he would require each subject to cancel each sheet *correctly* before time was taken, thus making speed the *only* variable.

reduction of time for the eight subjects from 47.6 to 33.7 secs. in the twenty sheets cancelled. For most subjects the errors remain constant; for two (F and H), however, they increase. The average for all subjects thus shows a slight increase in errors,—.6 to .9. The results are comparable to those of Kent,<sup>1</sup> who in twenty-one sheets distributed over seven days found for six observers an average reduction from 54.5 to 41.5 secs. and from .9 to .2 errors.

TABLE XI

## CANCELLATION LEARNING

Preliminary series with digit form. Figures show average time in secs. and average number of errors for the five sheets cancelled by each subject on each serial day. All errors are errors of omission.

	Subject	A	B	C	D	E	F	G	H
1.	Secs. ....	44.8	52.6	54.2	57.6	30.6	43.8	51.0	46.2
	Errors ....	.6	.6	.2	.4	.4	1.4	.4	.8
2.	Secs. ....	45.6	47.6	44.6	36.6	26.0	32.0	40.8	44.2
	Errors ....	.4	.2	.0	.6	.2	1.8	.2	1.0
3.	Secs. ....	37.4	44.2	46.2	35.0	27.2	26.8	35.4	34.6
	Errors ....	.4	.0	.0	.2	.4	.6	.4	1.2
4.	Secs. ....	33.4	41.2	41.8	34.4	25.4	21.2	34.2	38.0
	Errors ....	.4	.0	.2	.4	.6	3.8	.8	1.6

In series I, following the preliminary series, form P was used with subjects D, F, G, and H; form Q with subjects A, B, C, and E. Series II, in which each subject was given the form not used in Series I, followed Series I by a two-day interval. As has already been noted in the histories of the subjects, G refused to continue the work after the first day, and H after the fifth day of this series. The results for both series appear in Tables XII and XIII, and are represented graphically in the curves of Figs. 8-15. In these curves the average values of both times and errors for each day of the series are shown. Results of Series I are represented by a full line; of Series II, by a dotted line. In examining the curves for evidence of learning, it must be borne in mind that a decrease in time does not necessarily indicate a better performance unless it is accompanied by a decrease in errors. A decrease in time with an increase in errors

<sup>1</sup> *Op. cit.*, p. 388.

or the reverse, a decrease in errors with an increase in time, is equivocal, for there is no way of establishing the value of an error in units of time. Cattell and Farrand<sup>1</sup> suggest adding a proportionate amount of time for the omitted characters in their form, but Wissler<sup>2</sup> thinks that this is not satisfactory. Examination of the curves shows many instances in which a diminution of one factor is accompanied by an increase in the other. This relation is especially evident with subject F, who worked very erratically, sometimes with careless speed, sometimes with laborious care. Note in the curves, for example, Series I, day 5,<sup>3</sup> All of the subjects, however, may be said to have acquired skill in the operation. There is no increase at the end of the series in any case for either factor. The improved performance is however, indicated for some observers principally by a decrease in the number of errors, for others principally by a decrease in time, and for others by diminution of both errors and time. A and E, in both series, and G, in the only series he performed, show learning by a decrease in both errors and time. In both series, C improves chiefly by decreasing the time, B, by decreasing the number of errors. H profits most by lessened time in the first series, but improves in both respects in the second; while D profits chiefly by a diminished number of errors in the first series and improves about equally with respect to both factors in the second, although the improvement in neither case is great. F alone completely reverses his type of learning, showing in Series I a great increase in accuracy with very little decrease in time, in Series II, considerable improvement in time without much change in accuracy. This change followed immediately a reproof at the end of Series I, in which the subject was chided for his carelessness in doing the work. He thus in Series II keeps the error small throughout, consuming much more time in the work.

<sup>1</sup> *Psych. Rev.* 3, 1896, p. 641.

<sup>2</sup> *Psych. Rev. Monograph*, No. 16, 1901, p. 27.

<sup>3</sup> Other examples are: Series I: B-7, 9; C-3, 6; D-11, 12; F-5, 6, 7; G-2, 7; H-11. Series II: D-4, 8; F-3, 6, 8. The letter refers to the subject, the number to the day on which the instance occurred.

TABLE XII

CANCELLATION TEST: SERIES I

Figures show average time in secs. and average number of errors for five sheets cancelled by each subject on each serial day. Errors made by cancelling the wrong character are preceded by a "+". All other errors are those of omission.

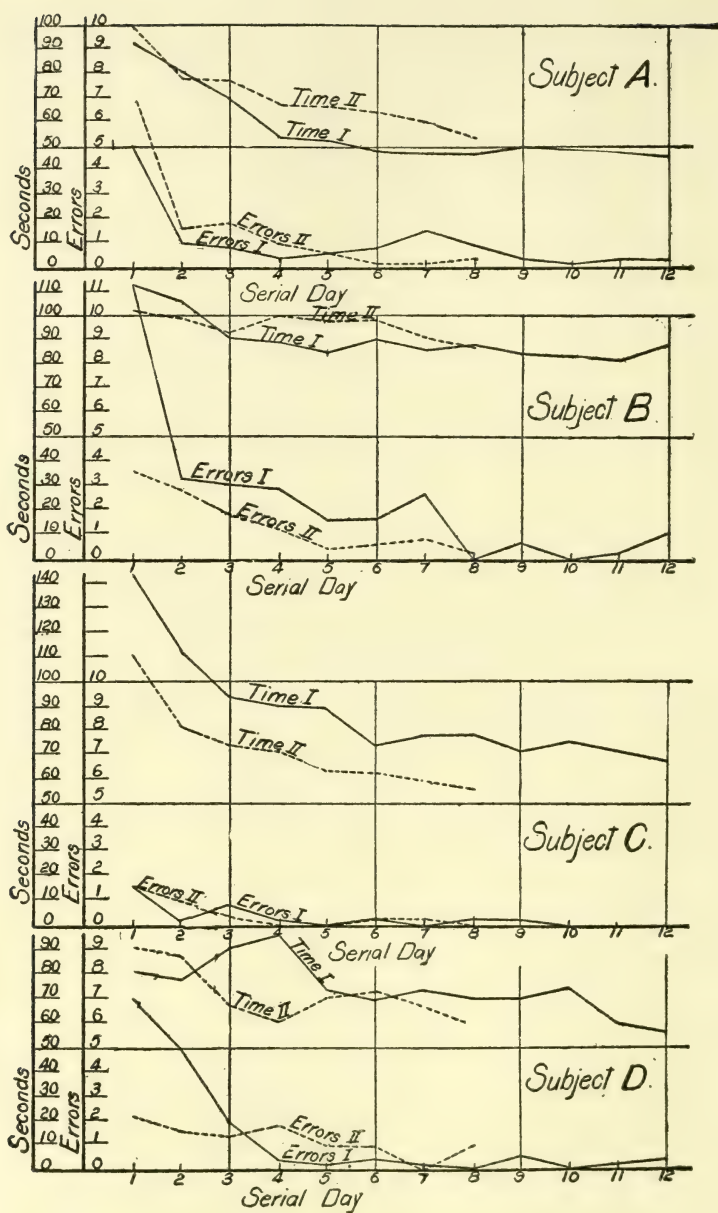
Serial Day	Subject Form used	A	B	C	D	E	F	G	H
		Q	Q	Q	P	Q	P	P	P
1.	Secs. ....	92.3	113.4	145.6	81.0	77.2	72.2	104.8	90.8
	Errors .....	4.8+.2	5.8+5.4	1.2+.4	7.0	8.4+4.4	5.4+2.8	1.8	2.6
2.	Secs. ....	80.4	106.2	113.0	88.6	64.0	62.6	82.6	69.0
	Errors .....	1.0	2.0+1.2	.2	5.0	2.2+.6	5.6	2.6	1.4
3.	Secs. ....	69.6	91.4	94.8	91.0	53.6	61.6	71.8	58.2
	Errors .....	.8	2.4+.6	.8	2.0	1.0	2.4	1.4	.2
4.	Secs. ....	63.0	89.6	90.0	96.8	46.0	61.4	65.8	61.8
	Errors .....	.4	2.6+.2	.0+.2	.4	.8+.2	.8	.4	.4
5.	Secs. ....	52.6	85.6	89.0	74.0	40.2	54.4	56.8	64.8
	Errors .....	.6	1.4+.2	.2	.2	.8	.6	.6	.6
6.	Secs. ....	48.4	90.6	74.6	70.0	34.2	61.2	55.2	49.0
	Errors .....	.8	1.4+.2	.2	.4	1.2	2.8	.2	2+.2
7.	Secs. ....	47.6	86.0	78.4	76.0	38.6	67.0	50.2	53.0
	Errors .....	1.6	2.4+.2	.2	.2	1.0	2.0	1.0	.6
8.	Secs. ....	47.4	88.6	78.0	74.2	30.8	63.2	42.9	42.4
	Errors .....	1.0	.2	.2	.2	.6+.2	.4	1.0	.4
9.	Secs. ....	50.8	84.6	71.4	71.8	28.4	57.8	45.0	39.2
	Errors .....	.4	.6	.2	.6	.6	.4	.6	.2
10.	Secs. ....	49.4	83.4	75.4	75.8	30.4	62.2	43.2	39.2
	Errors .....	.2	.2	.2	.2	.2	1.0	.2	.2
11.	Secs. ....	48.6	81.8	71.6	62.8	28.2	67.2	41.0	32.6
	Errors .....	.4	.2	.2	.2	.6	.6	.2	.8
12.	Secs. ....	47.8	87.8	67.7	58.2	26.6	60.8	42.0	35.4
	Errors .....	.4	.8+.2	.4	.4	.4	.4	.4	.2

TABLE XIII

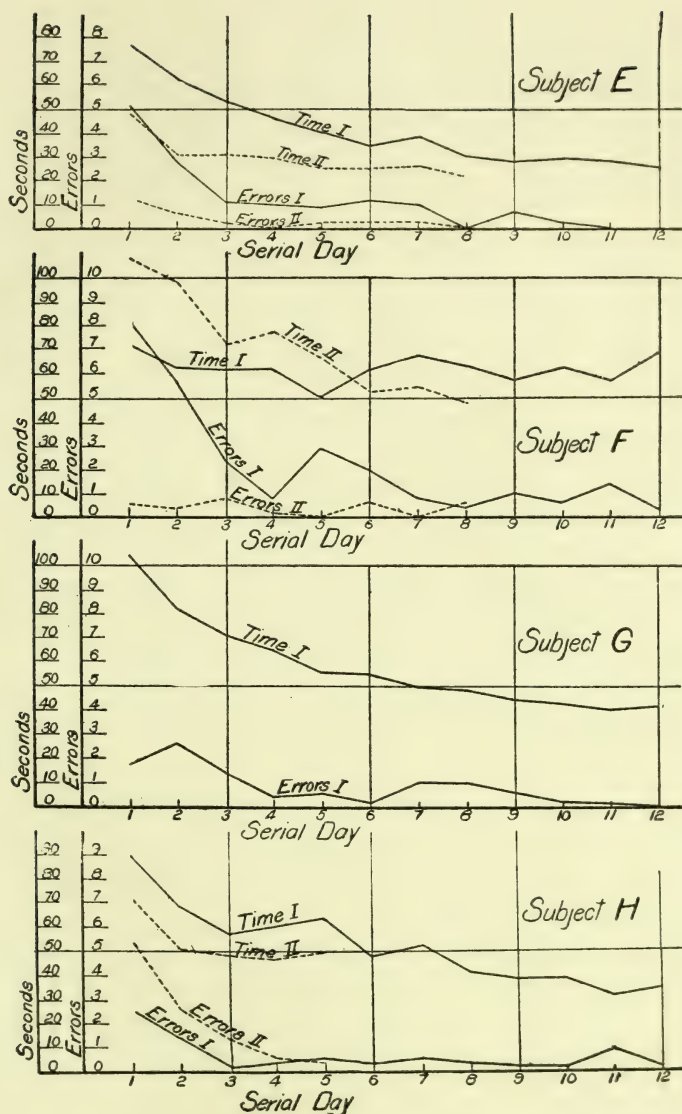
CANCELLATION TEST: SERIES II

Figures show average time in secs. and average number of errors for five sheets cancelled by each subject on each serial day. Errors made by cancelling the wrong character are preceded by a "+". All other errors are those of omission.

Serial Day	Subject Form used	A P	B P	C P	D Q	E P	F Q	G Q	H Q
1.	Secs. ....	99.2	102.2	111.4	91.4	48.6	107.2	125.2	72.8
	Errors .....	4.4+4.4	3.6	1.6	.8+1.4	1.2	.6	.4	.8+4.6
2.	Secs. ....	78.4	99.4	82.8	88.8	30.0	98.0		51.6
	Errors .....	1.6	2.6+.2	1.0	1.6	.6	.4		2.6
3.	Secs. ....	77.8	93.4	74.6	68.4	30.6	72.4		49.8
	Errors .....	1.8	1.8	.4	1.4	.2	.8		1.4
4.	Secs. ....	67.2	100.8	71.8	61.2	29.0	77.8		47.6
	Errors .....	1.0	1.2		1.8		2.0		.6
5.	Secs. ....	67.8	98.4	63.0	71.8	25.0	66.2		50.2
	Errors .....	.6	.2+.2		1.0	.2			.4
6.	Secs. ....	64.4	98.4	62.0	73.0	25.4	52.6		
	Errors .....	.2	.4+.2	.2	1.0	.2	.6		
7.	Secs. ....	60.8	91.6	59.8	68.4	26.4	54.0		
	Errors .....	.2	.8	.2		.2			
8.	Secs. ....	54.0	87.0	56.2	60.0	22.4	49.6		
	Errors .....	.4	.2		1.0		.6		



Figs. 8-11. Cancellation Test. Time and error curves for both Series I and II for subjects A, B, C, and D. Curves for single series show learning. Comparison of two curves shows transfer of practice.



Figs 12-15. Cancellation Test. Time and error curves for both Series I and II for subjects E, F, G, and H. Curves for single series show learning. Comparison of two curves shows transfer of practice.

c. *Character of the operation.* In considering a test of learning we must deal with the qualitative as well as the quantitative side. Not only must we ask, "How much was learned?", but also, "Just what was learned?" Not only is it of interest to note the acquisition of skill, but it is important to examine also the character of the operation on the conscious side. In some cases, for example, in the discussion of the transfer of training below, the former is dependent in a large measure upon the latter explanation. For this reason notes were kept of the manner in which the subject proceeded in cancelling the forms, and of the subject's replies to such questions as, "Do you remember where the characters are?", "How do you remember where they are?" The notes on procedure and the significant introspective replies of the subjects are summarized below. Where the value of an answer might be dependent upon the amount of suggestion in the question both are given *verbatim*, as far as is possible in a limited space. Some of the replies are so naïvely simple that the introspective accuracy of the subject may be questioned. It should be remembered, however, that at the same time these subjects were giving surprisingly good introspections of the largely kinesthetic consciousness involved in learning the maze.<sup>1</sup>

<sup>1</sup> In this test and in the maze test following we are to examine consciousnesses that accompany the formation of motor habits. For the sake of clearness it is worth while to state definitely at the outset the exact sense in which certain descriptive terms will be used in relation to the particular type of consciousness described.

We are considering in these tests *movements*, which may or may not be accompanied by *relevant consciousnesses*. When a movement is accompanied by relevant consciousness, it is an *action*, and the accompanying consciousness, an *action consciousness*. When a movement is not accompanied by relevant consciousness, it is an *automatic movement* or an *automatism*. Thus, when it is said a "performance becomes automatic" or that "consciousness lapses", it is meant that a consciousness relevant to the movement, *i.e.*, an action consciousness, fails to reappear when the movement is repeated.

The action consciousness, moreover, although only part of a larger total consciousness, is itself complex. It includes certain processes which are relevant to those movements that materially increase or decrease the time of performance of the given operation, and may thus be said to be important for learning. Processes that are relevant to movements which

[The following abbreviations are used: S = subject; Q = question; A = answer; V = statement volunteered without questioning.]

SUBJECT A. *Series I.* S at first goes over every line from left to right. Later, omits going over last line, and finally omits going over first and last three lines except to cancel such figures as occur immediately.

Day 12 (last day). Q. What do you do to find the figures? A. Just look for that [points]. Q. Do you remember where they are? A. No, sir, I don't think I remember at all. Q. Do you know how many are on each line? A. No.

*Series II.* Procedure as before.

Day 4. Q. Are you beginning to know where they are? A. No, sir. I have to look until I find them.

Day 8 (last day). Q. Tell me just how you find them? A. I look for a square with the right corner out. Q. Do you have to go over the whole sheet to get them all? A. Yes, sir; I go over every line, all but the last two. I know there aren't any there.

SUBJECT B. *Series I.* S goes very slowly from left to right over one line and back from right to left over the next; and so on over the whole sheet, until at the end he omits the last two lines. He often pauses for considerable intervals, as if having difficulty in maintaining attention.

Day 6. V. I think I've found out what's the matter. I've got my mind on the end of the pencil and not on the line.

Day 12. (last day). Q. How do you find them? A. I know about where to find them in the line. Q. When you get to each line, do you think how many there are in it? A. No, sir; not exactly. But in general it appears like I have an opinion where they would be located on a sheet. [In reports on the maze, S identifies 'opinion' with visual and perhaps verbal imagery.]

*Series II.* Procedure as before. Finally, first line and last two lines are not gone over.

Day 2. S reports he has no notion where figures are.

Day 5. Q. How do you remember where they are? A. By where they're located. Q. How do you know that? A. By the lines. Q. Do you know how many are on each line? A. No.

decrease the time of learning are spoken of as *cues*, and these cues may be either sensory or imaginal, perceptual or memorial. If, then, *e.g.*, in learning the maze, the right movement always occurs immediately after the perception of a landmark, it can be said to be touched off by a perceptual cue; if it occurs at the recurrence of an idea of movement at a given time (as is often the case in kinesthetic memory) it can be said to be set off by a memorial cue. Other processes in the action consciousness may not be relevant to the particular character of particular movements, but to the general continuity, speed, uniformity, care, and so forth of the movement; these are spoken of as carrying the *Aufgabe* to keep moving, to move quickly, to be careful, and so forth. They are not regarded as cues, although all the cues may be said to carry a part of the *Aufgabe* to perform the operation perfectly.

Day 8 (last day). Q. Do you know where they are placed? A. No, sir. Q. Take this paper and mark on it as well as you can where you think they are placed. A. I couldn't do it. I wouldn't have any idea where they were. Q. How do you find them when you cross them out? A. I just look for them.

SUBJECT C. *Series I.* S goes over every line from left to right. Later hurries over last two lines, but does not omit them.

Day 8. After four perfect sheets, S makes one error. V. I think I skipped one.

Day 11. Q. How do you find them? A. I look at them all and mark those that have a cross to the left and one underneath.

Day 12. (last day). Q. How do you find them? A. I go over every line except the last. I don't look at that so much.

*Series II.* Procedure as above, except that last two lines come to be completely omitted.

Day 1. Q. Do you know where they are? A. I know where a couple of them is.

Day 4. Q. Do you remember where they are? A. I generally know where they lay at. Q. Do you know where they all are? A. No. I just know when I get there.

Day 5. Q. How do you find them? A. I look for those with opening at one side underneath.

Day 6. After making an error: V. Seems like I know I missed one. Q. And yet you don't know where they are? A. Oh, yes; I know about where they are, that's how I knew I missed that one.

Day 8 (last day). Q. Do you go over every one of them to find them? A. Oh, not every one. I tend to know about where they are. I sort of expect them.—S is, however, unable to attempt to draw the arrangement on a sheet of paper.

SUBJECT D. *Series I.* S goes over all of each line, from left to right, and continues to do so throughout, although he hurries over the last two lines toward the end. He is mute much of the time.

Day 11. Q. What do you do to get all the figures? A. Just look. Go over each line.

Day 12 (last day). Q. How do you find them? A. I go over each line and check them off when I come to them.

*Series II.* S goes laboriously over all of every line as before.

Day 5. Q. Do you know where they are? A. I have the picture [of the characters to be cancelled] in my mind and cross them out when I come to them.

Day 8 (last day). Q. How do you find them when you go to cross them out? A. I just cross them out when I come to them. Q. Have you any idea of how they are arranged on the sheet? A. No.

SUBJECT E. *Series I.* On second day S begins to go irregularly from top of sheet to bottom, cancelling two or three lines at once. He improves in this method throughout, until at the end, he spends very little time going over unnecessary parts of the sheet, except for a few lines in the middle. He seems, however, seldom to repeat the same order.

Day 12 (last day). Q. Do you know where they are? A. Most of them; all but some in the middle. Q. How do you find them? A. I know about where to look for them and when I see them I cross them out.

*Series II.* Procedure exactly as above.

Day 1. V. Runs something similar to other figure. Can locate it pretty easy by remembering how the other went.

Day 3. Q. Have you learned it? A. They follow about the system of the other one [Series I]. Still you have to watch because it's a different figure, and the other figures kind of lead you off. First you check those three, then two, then three in the next row, then three, three, two, one, none, three, three, two, one. [This is correct for the number in the rows, except for one omission.] I don't count them because it takes too much time.

Day 8 (last day). Q. How did you do it? A. I have to watch. I know the arrangement pretty well. I count them off by threes.—S is asked to draw the positions of the characters cancelled on a sheet of paper. This he does with five omissions, placing twenty characters with approximate, although not exact, correctness. He does this quite rapidly, counting them off as he does so.

SUBJECT F. *Series I.* At first S goes over each line, from left to right, slowly. Later goes more rapidly and finally omits last two lines. He seems careless and often misses a whole line. At the end or in the middle, he stops at times and reviews his work, skipping about at random, and hunting for characters missed. Sometimes he spends 30 secs. in reviewing.

Day 12 (last day). Q. How do you find them? A. I know just about where they are located. Q. How do you remember where they are? by the way they look? by the number in each line? A. No, I know just about how they are located, if I don't go too fast; then I get mixed up. Q. How can you tell where they are, though? A. Just go ahead.

*Series II.* On the first day S goes over all but the first and last two lines, although he will not state that he finds the form similar to last. Thereafter procedure is as before.

Day 1. Q. What helps you to do better than you did with the old form at first? A. Better attention, I guess.

Day 5. V. I know where they're located, but I miss them once in a while. First three rows has got three in a row to cross out, one row's got two, one row one, and there's a pair together.—On one sheet done in quick time for this S, he states that he knows that he has made one mistake, and finds it after eight seconds, thus getting a perfect sheet.

Day 8 (last day). S gives appearance of knowing location of characters, not completing a line when he has crossed out the requisite number for that line. Q. How do you find them? A. I know how many there are in each line and find them that way.—S is asked to indicate on a sheet of paper the arrangement of the characters. He places 23 characters, about the right number, on each line. The first half of the sheet is poorly arranged, the second half quite well. Q. Do you do the form by thinking of some such plan as this? A. Yes.

SUBJECT G. *Series I.* S goes over each line, from left to right, at first. Later, he omits the unnecessary parts of the last four lines.

Day 2. V. I was thinking of memorizing the number on each line.

Day 5. V. I'm beginning to commit the number on each line to memory.

Day 12 (last day). Q. How do you remember the form? A. I remember how many there are on each line. I never counted up, though I say to myself for the lines, "There's one, two, three, then there's three, and then two, and then one." I know how many there ought to be when I look at the line.

SUBJECT H. *Series I.* At first S goes over all lines, from left to right. Later, he goes over all but the last two, either from left to right or from right to left, and sometimes does not complete line after requisite number have been marked. Looks over lines while holding pencil high in air, and appears by pencil movement to anticipate the position of the characters.

Day 10. Q. How do you find them? A. I know the number on some lines. I always have to look for them. Sometimes I get confused because I forget them.

Day 12 (last day). Q. How do you remember them? A. I remember the number in some lines. In a few lines there are three, and somewhere there is only one. I get confused in the middle.

*Series II.* Procedure throughout like procedure in Series I.

Day 1. Q. Are you beginning to know where they are? A. Yes. Aren't these the same arrangement? I think it because there are three here [points to first three].

Day 2. V. I'm pretty sure the two or three top lines are exactly the same as the last form. I'm not sure all the way through.

Day 4. Q. Do you know where they all are now? A. I know the number on each line.

From the foregoing records, we may conclude that the course of learning is essentially as follows: The subject, while becoming familiar with the appearance of the character to be cancelled, proceeds over the entire form line by line. The first evidences of learning appear both in reduced time and reduced number of errors, coming apparently as a result of increased familiarity with the character. When the subject is perfectly familiar with the appearance of the figure to be cancelled, he may begin to omit lines and parts of lines in which the character does not occur. This he does first at the beginning and end of the form, and last in the middle. Later he may come to remember, either in verbal or visual terms, the number of characters to be cancelled in each line, and follow each line along until he has crossed out the requisite number, or he may become familiar with the grouping upon the sheet, and cross out at once the mem-

bers of a group of two or three, whether on the same line or on adjacent lines. These last two methods are apt to be combined, and the subject seldom applies the same method consistently throughout an entire sheet.

It is quite probable that the process of localization may be resolved into two factors,—a gross factor by which the subject finds the general location of the symbol to be crossed out, and an exact factor, by which he determines the precise position of the character.

The first factor, the general localization of the character, appears at first as a visual-motor adjustment, which enables the subject to follow along from left to right on one line, and back from right to left on the next. Subject D never passed beyond this stage. Later the subject may come to go over all of the lines except parts of the first and last lines. Whether the omission of these parts depends upon a visual, verbal, or kinesthetic cue, the introspections do not show. Subject A progressed only thus far. Next appears a recognition of some or all of the characters when they are cancelled. This is reported by B, C, and E and probably existed for at least F and H in addition. It is apparently the response to a visual perception, and may be assumed to be organic in as far as it involves the feeling of familiarity. E, F, G, and H all came to remember the number of characters in each line. With G the recollection appears to have been verbal; with E and H it was probably either verbal or visual; with F it may have been hand-kinesthesia. The most advanced stage of gross localization seems to be in the grouping of the right characters, a method acquired only by subject E, for whom the groups fell partly in the lines and partly across the lines, and were apparently carried visually, although it is possible that the localization of the whole group upon the sheet was kinesthetic. The writer, who tried cancelling the forms after having become very familiar with their appearance and who was thus able to do them in less time than any of the subjects, also used the group method. For him the gross localization of the groups was carried by eye-strains

and organic sensations in the chest and shoulders. This cue, however, did not enable him to locate the groups except very roughly, say within an inch; all more accurate localization was entirely visual. The reports of all the subjects indicate that for them as well accurate localization of the character was visual. The character might be known to lie in a given region in a number of ways; the identification of its exact position was, however, always visual.

It must be observed that accurate localization is by far the most important part of the operation for learning. A gross localization may come in verbal, visual, kinesthetic, or attitudinal terms, or may be even automatic, but the operation is by no means accomplished. Sometimes the subjects (especially F) would search over an area of not more than ten square centimeters for a character that it would take two or three seconds to find. The important factor is accurate visual localization.<sup>1</sup>

It is not impossible, of course, that the operation would have become largely a kinesthetic habit had it been continued for a longer time, but that it could be acquired as completely motor any more easily than the operation involved in the playing of a simple piece on the piano in the dark or writing neatly with the eyes shut, the writer doubts.

*d. Transfer of practice.* In her work with these forms, Kent compares the performance at the beginning of her work with the performance in cancelling another character, similarly arranged, but in a different set of symbols, after two months of work, during which the subject cancelled on still other forms a character in this same arrangement.<sup>2</sup> She finds that out of thirty-eight cases,—these are comparisons of single trials, not of series,—“fourteen show a gain in speed with a loss in accuracy, and

<sup>1</sup> The exact factor has been called ‘important’, (1) because it is essential to the performance of the operation and (2) because it is more subject to improvement with practice. A form of gross localization is found ready-made, in the almost universal habit of reading from left to right along successive lines. The exact localization, the selection of the right character, is, however, a new act (with unfamiliar figures) and already implies learning during the course of the experiment, if it occurs at all.

<sup>2</sup> *Op. cit.*, p. 395.

eight show a gain in accuracy with a loss in speed," the improvement in these cases being indeterminate, and "sixteen cases show an improvement in both speed and accuracy, while in no case is there a loss in respect to both variables." From this she concludes that "practice effect gained in one kind of work appears to be to some extent transferable to another kind of work which differs from the first in its perceptual but not in its motor aspects."<sup>1</sup> She observes, however, that "it is obvious that the gain may have been due in part to general adaptation as well as to practise transfer."<sup>2</sup>

In the present work it was planned to eliminate the effect of "general adaptation," in the first place, by not beginning the cancellation tests until the subjects were accustomed by other tests to the general kind of work expected of them during the experimental hour; and, in the second place, by introducing a preliminary series of forty trials on the digit form, during which the subjects might become accustomed to the particular procedure required in this test.

The effect of practice, as shown by the two complete series with forms P and Q, can be seen immediately by reference to the curves of Figs. 8-15. With subject F (and also with G, if we consider his one day's work as a sufficient indicator) a decrease in error is offset by an increase in time; with H the reverse is the case; with D, first an increase in accuracy is opposed by an increase in time and later the reverse is the case. All these cases are then indeterminate. An improvement in both variables occurs markedly throughout the course in the case of subject E, and also in the first two days with subject B. Subject C shows an improvement in time with approximately no change in the amount of error. These three can probably be said to show improvement with practice. A, on the other hand, is poorer in both respects for the first five days, after which he becomes indeterminate. Thus, of eight individuals, three (B, C, E) appear to indicate transfer of practice, four (D, F, G, H)

<sup>1</sup> *Ibid.*, p. 409.

<sup>2</sup> *Ibid.*, p. 395.

are indeterminate, and one (A) gives negative evidence of its occurrence.

The question arises: Are these differences due to any difference intrinsic in the forms themselves? The introspections taken on the two forms brought out the fact that the perceptual processes in the early stages of recognition were essentially different. With form P, the subject identifies the character by recognizing the open corner at the lower right hand corner. With form Q, he perceives first the arm on the left with no arm on the right, and then, as a second step in the process of identification, notes the three legs. Sometimes, but not often, the order is reversed, but in any case, it is two or three days before the subject reports, "I see it all at once," a report, taken to mean a grouping into a perceptual whole. It is possible that form Q is thus really more difficult than form P, an unfortunate occurrence, since it happens that all the cases in which form Q was used last, the cases which would form the most reliable index of improvement with practice, are indeterminate cases. Of the four cases in which form P was used last, three appear to indicate that practice has been advantageous to a reduction in both errors and time, and one that it has been disadvantageous; but it now appears possible that the three to one difference may merely be a measure of this difference in difficulty of the two forms. At the best the case is not strong for a transfer of practice,—although the conditions are not greatly different from those of Kent. Possibly her results may have been due to a "general adaptation", as she suggests.

The reason for this absence of any marked evidence for the transfer of learning becomes more evident as soon as we examine the introspections of the subjects. The writer anticipated improvement in Series II because it seemed that a kinesthetic habit formed in Series I would persist advantageously in Series II, even though the visual perceptual cues were altered; and such a persistence Kent apparently assumes to be the case. The introspections, however, have shown that the operation never came to be essentially kinesthetic. The general adjustment to the

particular work, the holding of the pencil so as not to obscure the sight of the lines and yet so as to mark quickly and readily, the keeping of the place on the form, the marking with the pencil, matters of this sort became kinesthetic or automatic very early, probably in the preliminary series, and thus do not affect any comparison of the two main series; but the actual localization of the characters remained predominantly visual, and being visual, only such parts of the visual perception as remained unaltered with the change of form, that is to say, the grouping of the characters, could be effective for an improved performance in the second series. In this connection, it is notable that the one observer (E), who grouped characters over almost the entire page, instead of following out the lines in order to find them, shows by far the greatest improvement in Series II.

*e. Comparison of subjects.* It is thought worth while to compare the performances of the different individuals in the cancellation work. Accordingly in Table XIV the relative ranks of the subjects in different factors of the test are given. The ranks are based upon the average times and the average number of errors for each subject in Series I, which was the series completed by all individuals. Ranks according to errors and to time are given separately, as it is impossible to combine these two factors satisfactorily. The general performance of the subject is perhaps, however, indicated by the "combined rank," which is based upon the other two rankings,—for time and for errors. The observers were also ranked according to "type of performance," by which is meant the means employed in localizing the characters. In gross localization, it will be recalled (pp. 45f), we recognized five stages: (1) examining all of every line, (2) omitting lines or parts of lines, (3) recognizing characters, but not necessarily anticipating their position, (4) remembering the number of characters on each line in verbal, visual or kinesthetic terms, and (5) grouping characters on the sheets in verbal, visual, or kinesthetic terms. The rank on this basis indicates the number of steps in the typical series through which the subject progressed.

TABLE XIV

## RANK OF SUBJECTS IN CANCELLATION TEST

Basis of Ranking	Subjects							
	A	B	C	D	E	F	G	H
Average time.....	3	8	7	6	1	5	4	2
Average number errors.....	4	8	1	6	5	7	3	2
Combined time and errors.....	3.5	8	5	6.5	2	6.5	3.5	1
Type of performance.....	7	5.5	5.5	8	1	3	3	3

It should be noted in the table that with the exception of subjects C and E the rank position based upon the errors made and that based upon the time do not differ greatly (69.1 % correlation), the subjects appearing to strike about the same balance between the two possible lines for improvement. Subject C, a notable exception, always went very carefully and slowly over his form with fairly good attention. He thus ranks first in accuracy and next to the last in time. It may also be observed that there is some similarity between the ranking according to type of performance and that for time (58.8% correlation). The type of performance and the errors do not appear to be so closely related (13.4% correlation). It is probable that the earlier factors of localization upon which the type is based are those which allow the subject to "speed up" and to hesitate less, but do not make him more sure of avoiding certain persistent errors that remain for some time.

The evidence for the transfer of practice is too dubious for us to attempt to rank the subjects in this regard. They can, of course, be ranked upon the improvement made in respect to time and accuracy separately. Based upon the actual number of seconds or errors saved in the first five trials of the second series from the first five trials of the first series, the orders are as follows: For improvement in time (best subject first), C, E, H, D, B, A, G(?), F; for improvement in accuracy, F, B, E, D, C, G(?), H, A. There is little agreement (a correlation of 11.8%) between the two kinds of improvement.

## 3. MAZE TESTS

The cancellation test served for the study of the learning of an operation, which, when learned, involved a variety of

imagery, sensory cues, and automatisms. The maze was selected for learning because it presents an operation that is essentially motor in its performance, that is to say, the accompanying consciousness very soon becomes kinesthetic, and then soon thins out as the movement becomes automatic. Especially is this true when the subject is deprived of the use of vision while learning the maze.<sup>1</sup> The series were therefore arranged so that the subjects might finally traverse the maze without the use of vision, thus making more likely the predominance of the motor character of the performance.

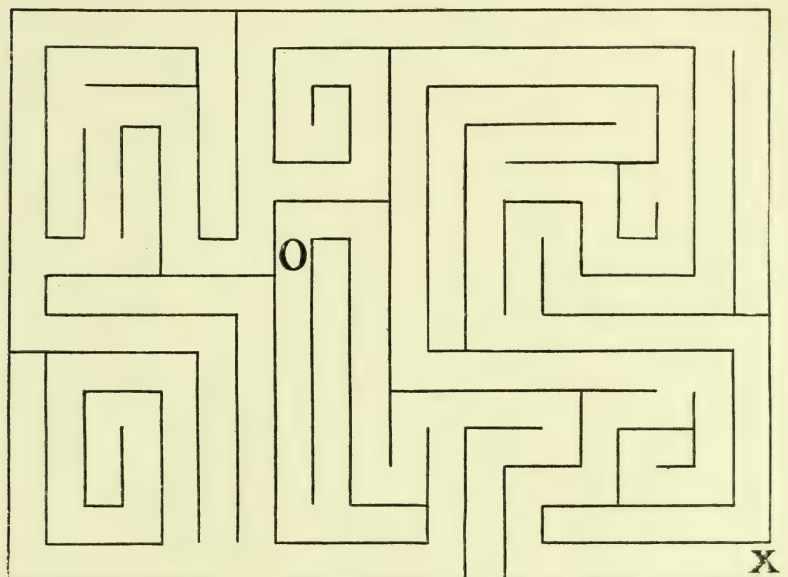


Fig. 16. Maze, used in preliminary practice series. Subject goes from O to X.

*a. Procedure.* Three series were given. In the first the printed maze form, shown in Fig. 16 was used.<sup>2</sup> The subject

<sup>1</sup>A detailed analysis of the course of consciousness in human maze learning has been made by L. M. Day and the writer, and will be published shortly. The general course of the different imaginal and perceptual factors appears in a preliminary report on this work, *Psych. Bull.*, 9, 1912, p. 60.

<sup>2</sup>This is the form of maze used by Kent, although only one-half the size of hers.

was required to trace his course directly upon the form with a pencil, starting at "O" and finishing at "X". Since the pencil left a trace he had the advantage of seeing his former mistakes in the same trial. The subject used five sheets at a sitting. The series consisted of four sessions, forty-eight hours apart. Although indicating learning, it was intended only to be preliminary, to familiarize the subjects with the character of the maze problem.

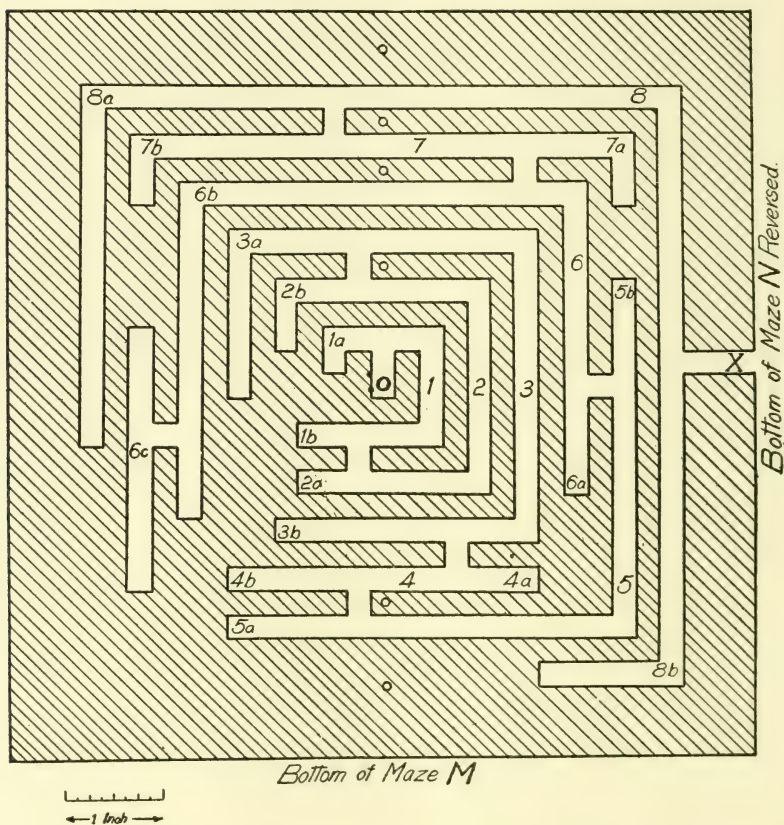


Fig. 17. Maze M, used in Series I. This maze, inverted by turning bottom-side up from right to left and then rotated through  $90^\circ$  in a counter-clockwise direction, forms Maze N, used in Series II. Numbers indicate corresponding passages in either maze. Correct course is "O-1-2-3-4-5-6-7-8-X." Slots in maze are as deep as broad,  $\frac{1}{4}$  inch. Figure  $\frac{1}{2}$  full size.

In the first main series, maze M was used (Fig. 17). This maze was cut from fiber,  $\frac{1}{4}$  inch thick. Passages, doors, and walls were all  $\frac{1}{4}$  inch wide. Five small holes along the median vertical axis (see figure) were drilled, so that the maze could be placed upon a wooden base, in which five pins, projecting through the holes, held the maze firm and kept it from springing. In using the maze, sheets of paper with holes punched to correspond to the pins, were placed first upon the base. The maze was then placed on the pins and above the paper. The course was traced by the subject with a pencil, so that a record was left upon the paper, the subject starting in the center at "O" and being instructed to "get out" as soon as possible, the exit being at "X".

The maze was made simple so that the subjects could easily learn to traverse the course without seeing what they were doing. It was not thought, however, that they could readily learn the maze without ever seeing it at all. The series were, therefore, arranged so that trials with the maze in sight should be alternated with trials with the maze obscured from view, the number of trials with the maze in view being regularly reduced, until the course could be followed rapidly and correctly without ever being seen. As in the other tests, five trials were given every other day. On each of the first two days, four trials were made with vision and the final one, without; on the third and fourth days, three trials were made with vision and the final two, without; on the fifth and sixth days, two trials, with vision and three, without; and so on until the ninth day when all the trials were without vision.<sup>1</sup> Thereafter the maze was not seen. The series was continued to include twelve days, even with those subjects who learned most rapidly, thus giving equal amounts of practice to all the subjects before continuing with maze N.

The maze was obscured by the use of the curtain described in the account of the kinesthetic memory test (p. 28), thus avoiding the distraction incurred by blindfolding.

After the completion of series I, the maze was reversed by

<sup>1</sup>Two exceptions to this procedure occur; one, with subject A, day 2, was before the form of the series had been fixed; the other, with subject D, day 6, was the result of a mistake.

being turned bottom side up and rotated through  $90^\circ$  in a counter-clockwise direction. The maze in this position will be known as maze N.

The subjects, now thoroughly familiar with the maze problem from the series with maze M, were required after an interval of from one to five days to learn maze N without seeing it at all. As before, five trials every other day were given. The series was continued in most cases until the subject's average time was reduced below ten seconds.

*b. Preliminary series.* The results with the printed maze form, used to familiarize the subjects with the maze problem in general, are summarized in Table XV, the figures being averages for the five trials given on each day. It will be observed that all subjects learned this maze readily, and all decreased the average time uniformly from day to day, with the exception of D, who was apt to be erratic.

TABLE XV  
PRELIMINARY MAZE. FIGURES SHOW AVERAGE TIME IN SECS. FOR FIVE TRIALS ON EACH DAY

Serial Day	Subjects							
	A	B	C	D	E	F	G	H
1	169.4	108.2	111.8	97.6	53.0	158.0	94.6	75.6
2	53.6	92.4	51.6	52.6	17.8	24.8	46.8	41.8
3	26.6	30.4	50.6	126.2	14.6	12.0	27.0	29.2
4	22.2	18.6	29.4	49.0	15.4	12.8	22.0	22.8

*c. Series with maze M and maze N.* There appear below the records of the learning of the maze by each subject. It has seemed best to combine the times, the notes on the subjects' performance, and the reports of the subjects, in order that their relation may be more evident. Occasionally interpretations or comments by the experimenter are also included. Since the best illustration of the formation of the motor habit is given by maze N, and since the subjects did not learn to give introspective reports of much value before the beginning of the second series, the reports of maze M will be made as brief as possible; only the points contributing to an understanding of the character of the performance will be included.

## ABBREVIATIONS USED

In order to economize space the following scheme has been adopted. The letters "a" to "e", placed in parentheses, are used under each day to indicate the five trials on that day. The numbers following them are the times of the respective trials in seconds. The letter "v" (= "visible") preceding a number indicates that in that trial the subject was allowed to look at the maze, *i. e.*, the visual perceptual cues were present. The letter "i" (= "invisible") indicates that in that trial the maze was obscured by the curtain, *i. e.*, the perceptual cues were presumably<sup>1</sup> only kinesthetic. The letters "v" and "i" are omitted in the consideration of maze N, since all the trials were without vision. After the times for the individual trials there is given for maze N the average for the day. For maze M the average would be without significance. Below the times are given observations, introspective reports, and comments, preceded by the letter of the trial to which they refer. Reports by the subjects were made after the trial referred to. Other abbreviations utilized are: S = subject; Q = question or statement by experimenter to subject; A = answer of subject; V = voluntary statement by subject without questioning or suggestion by experimenter.

Reference is made to definite positions or courses within the maze by means of the numbers shown in sketch of maze M (Fig. 17). In considering maze N, it is not necessary to conceive of maze M as reversed, but merely to take it as it is shown, for the relation of the parts is the same in the two mazes. The reader needs only to disregard the terms "right" and "left", which occur but seldom. It will be observed that the correct course through the maze would be designated as "0-1-2-3-4-5-6-7-8-x". An error always contains a letter, *e.g.*, "4-5-5b" or "7-6-6b-6c". It is possible to traverse the course in five sweeps of the arm, as follows: (1) 0-1-2, (2) 2-3, (3) 3-4-5, (4) 5-6-7-8, (5) 8-x. It can be done in three seconds, perhaps less.

In the following pages the reports for maze M and maze N are given together for each subject.<sup>2</sup>

*Subject A*

*Maze M.* Day 1. (a) v 20; (b) v 16; (c) v 13; (d) v 11; (e) i 178.

Day 2. (a) v 11; (b) i 58; (c) i 621; (d) i 748; (e) i 149.

<sup>1</sup> There occurred one instance in which an auditory perceptual cue, the sound of the moving pencil, appeared to be utilized. Doubtless tactual elements were regularly fused with the kinesthetic, although the subjects were unable to distinguish between the two. The use of the term kinesthetic is not meant to imply the complete absence of tactual processes.

<sup>2</sup> As a result of a mistake, for which the writer is partly responsible, the reports have been set up entirely in 11-point type instead of 8-point. This error has the effect of obscuring the summaries for each subject, which were intended to be left in the larger type. The casual reader will find the summaries for each of the eight subjects, separated off from the other matter by leads, on pages 58f, 61f, 67, 70f, 73, 76, and 78.

Day 3. (a) v 11; (b) v 11; (c) v 9; (d) i 76; (e) i 115.

Day 4. (a) v 12; (b) v 10; (c) v 9; (d) i 67; (e) i 51.

(d) Q. How did you remember that? A. I remembered a little bit how it looked. I knew I wasn't going back into the center again. Q. Did you always know what side the next turn would be on? A. No.

Day 5. (a) v 9; (b) v 7; (c) i 42; (d) i 42; (e) i 30.

(c) Q. Did you know the way? A. Pretty much. Q. Could you see how it looked? A. I didn't see it much. I could tell by the touch of the pencil.

(e) S proceeds slowly, feeling walls. "Touch of the pencil" evidently means feeling for doors.

Day 6. (a) v 8; (b) v 6; (c) i 87; (d) i 42; (e) i 26.

(c) Q. Do you remember it? A. Pretty well. I started the wrong way.

Day 7. (a) v 7; (b) i 28; (c) i 20; (d) i 18; (e) i 18.

(c) Q. How do you get out? A. I know to keep to the right all the time. One place where there are two gates together [probably 3-4-5], I try to think when I come to them.

Day 8. (a) v 7; (b) i 88; (c) i 40; (d) i 40; (e) i 25.

Day 9. (a) i 36; (b) i 61; (c) i 41; (d) i 29; (e) i 23.

(a) Q. How did you remember to do it? A. Just remembered. Q. Did you think what it looked like? A. No, sir. Q. Did you think how it felt to do it? A. Yes, sir.

Day 10. (a) i 26; (b) i 20; (c) i 16; (d) i 20; (e) i 20.

(e) Q. How did you know how to get out? A. I just feel with the pencil. Q. How do you know which way to feel with the pencil? A. Just from seeing it the first time; then I followed the way I thought it ought to go. Q. The way you remembered it looked? A. Yes, sir. Q. Do you know what the maze looks like while you are going through? A. No, sir; just think, and feel with the pencil. Q. But what do you think of,—the way it looked? A. No sir, just how to get out.—Note the contradictory answers. S is apparently quite suggestible. The last answer, which is not the suggested answer, would seem to indicate that the movement is becoming motor. S is asked to draw the maze on a separate sheet and does so, puzzling very much over the task and making at least five important errors. Q. Do you think now after drawing the maze that you could get out by remembering how the maze looks? A. No, sir; I get out by just following the pencil. I feel along and follow the pencil.—Evidently the operation is kinesthetic.

Day 11. (a) i 22; (b) i 15; (c) i 14; (d) i 21; (e) i 14.

Day 12. (a) i 12; (b) i 11; (c) i 10; (d) i 15; (e) i 16.

Maze N. Day 1 (a) 510; (b) 237; (c) 252; (d) 253; (e) 457. *Av.*, 341.8.

Day 2. (a) 96; (b) 183; (c) 57; (d) 65; (e) 43. *Av.*, 88.8.

S moves slowly and makes only three or four mistakes each trial.

(c) Q. How do you remember how to get out? A. Just try to study how to get out. Q. What do you think about? A. Don't know, sir; it just comes in my mind to try to get out. Q. Do you keep thinking about the way the maze must look? A. No, sir, not at first; after I've felt my way around I try to think how it looks—how to get out. 'Thinking how to get out' is perhaps equivalent to visual imagery for this subject.

(e) Q. Do you still do this by studying? A. I try to think how it looks after doing the other. I get out that way. Q. Do you think all the time you're trying to get out how it must look? A. Yes, sir.—The process must be largely visual.

Day 3. (a) 67; (b) 25; (c) 27; (d) 29; (e) 36. *Av.*, 36.8.

(a) Q. How did you remember it? A. Don't know, sir. Just felt my way.

(b) Q. Was that easier? A. Yes, sir. I tried a different way. Q. How was it different? A. Different passageways. Q. Was it different all over or just at one place? A. All over, sir.

Day 4. (a) 116; (b) 49; (c) 64; (d) 33; (e) 21. *Av.*, 56.6.

(e) Q. Do you think you know the way? A. I'm not quite certain where to turn.

Day 5. (a) 34; (b) 28; (c) 21; (d) 18; (e) 17. *Av.*, 23.6.

(e) Q. Do you know it pretty well? A. Yes, sir. Q. How do you remember it? A. I just feel with the pencil in the passage. Q. Does that tell you how to turn? A. No, sir. Q. What does? A. I just feel, if I can't get out one way, and then come back and go the other.

Day 6. (a) 24; (b) 25; (c) 19; (d) 19; (e) 15. *Av.*, 20.4.

(a) Q. How did you remember that? A. I just knew how to go. Q. Did you think how to turn at each door? A. No, sir; I just did it naturally. [Expression 'did it naturally' may be borrowed from rug-making introspections, given by S on preceding day.] Q. Didn't you know where you were going to turn before you did turn? A. No, sir. [Positive.]

(b) Q. How much did you think about that? A. I wasn't thinking much about it. Trying to keep going, that's all.

Day 7. (a) 20; (b) 19; (c) 15; (d) 13; (e) 16. *Av.* 16.6.

(e) Q. How was that? A. I keep feeling for the doors and

go to the left. Q. Is that all you do? A. Yes, sir. Q. Could you draw a picture of the way you go? A. I don't know [appears very doubtful].—Apparently the habit is largely kinesthetic, not yet automatic.

Day 8. (a) 15; (b) 11; (c) 14; (d) 11; (e) 10. *Av.*, 12.2.

(a) Q. Did you know that well? A. Yes, sir. Q. How did you know it? A. Just by the gates. I judged it. Q. Where the gates would be? A. Yes, sir.

(e) Q. How did you do it? A. Just came to the right, feeling for the passage. Q. Is that all? A. I can't explain it. I just kept on feeling for a door, but I can't explain how it was.

Day 9. (a) 13; (b) 9; (c) 13; (d) 9; (e) 8. *Av.*, 10.4.

(e) Q. What did you think about? A. I don't know. Just hunted for passageways. Q. When you made a turn did you think which way to go? A. I don't know.—S is urged to answer. A. Well,—it's just from practice,—doing it so often I get used to it.

Day 10. (a) 12; (b) 9; (c) 8; (d) 8; (e) 14. *Av.*, 10.2.

(e) Q. What helped you to get out? A. I don't know how to explain it, sir. It was just feeling for the gates and thinking where they must be from practice. Q. Did you think much about it? A. Not very much.

Day 11. (a) 9; (b) 11; (c) 8; (d) 8; (e) 7. *Av.*, 8.6.

(e) Q. How did you remember it? A. Just by feeling, sir, and practice. Q. Did you have to think much about it when you were going through fast? A. No, sir; not much. Q. Did you think about it at all? A. Yes, sir; I knew I was going through.

Day 12. (a) 9; (b) 8; (c) 9; (d) 7; (e) 6. *Av.*, 7.8.

(e) Q. What did you think about while you did it? A. I don't know, sir. Just think about how to get out. Q. Did you think how to make each turn, when you came to it? A. No, sir. I just kept along the side of the walks and ran into the passageways. Q. When you went through a passageway did you always have to think which way to turn? A. No, sir. I did it from practice.—S is asked to draw his course on a separate sheet. He does it poorly with three important errors. Q. Did you ever think that this maze was anything like the first one? A. No, sir. I knew it was different.

We may conclude then, that for subject A the operation with maze M was first of an uncertain character becoming largely kinesthetic about the ninth day. The movement does not become automatic, although quite probably those factors connected with

the determination of the course become less prominent than those carrying the *Aufgabe* to get through (cf. "trying to keep going", day 6). With maze N, the operation is accompanied by visual imagery at first. The kinesthetic factor enters as prominent about the third day, and the visual factor has largely disappeared about the sixth day. The process is perhaps already somewhat automatic, and becomes more so, although not entirely so, later. The *Aufgabe* appears to be carried consciously, perhaps in kinesthetic terms.

### Subject B

Maze M. Day 1. (a) v 20; (b) v 9; (c) v 7; (d) v 7; (e) i 178.

(e) V. That's more guess-work than anything else. Q. Did you have a picture of it in your mind? A. No, sir.

Day 2. (a) v 8; (b) v 7; (c) v 6; (d) v 5; (e) i 18.

(e) Q. Do you always know where you are? A. I have an idea where I am. Q. Do you think of how the maze looks? A. I had an imagination of it.

Day 3. (a) v 6; (b) v 5; (c) v 5; (d) i 77; (e) i 33.

Day 4. (a) v 6; (b) v 5; (c) v 5; (d) i 20; (e) i 60.

Day 5. (a) v 5; (b) v 4; (c) i 42; (d) i 33; (e) i 110.

(c) V. I was lost. I didn't get started right.—Has S already a motor habit which must go off continuously in order not to be broken up?

Day 6. (a) v 6; (b) v 5; (c) i 26; (d) i 48; (e) i 95.

(d) Q. How did you remember it? A. General imagination. General opinion of the way it is.—Continued questioning of S seems to indicate that a 'general opinion' means visual imagery, but this is not positive.

Day 7. (a) v 4; (b) i 45; (c) i 28; (d) i 13; (e) i 13.

Day 8. (a) v 5; (b) i 38; (c) i 10; (d) i 9; (e) i 7.

(e) Q. How do you remember how to go? A. I just have a general idea. Like looking at a picture in the dark when you can about half-way see it.

Day 9. (a) i 12; (b) i 12; (c) i 10; (d) i 20; (e) i 13.

(e) Q. How do you remember it? A. Just imagination. Q. Tell me what you mean by imagination. A. Looking at something in the dark, not clear dark; having a general opinion of it. Q. Does feeling help you to get through? A. No, sir; except as I can generally tell when I run by a gap.

Day 10. (a) i 85; (b) i 27; (c) i 32; (d) i 36; (e) i 20.

(a) V. When I make a mistake I try to go back to the center and get started right.

Day 11. (a) i 11; (b) i 16; (c) i 12; (d) i 12; (e) i 1.

(a) V. I had a very good imagination all through. Q. What do you mean by imagination? A. Just a general opinion of it; something like looking at a map.

Day 12. (a) i 12; (b) i 11; (c) i 10; (d) i 10; (e) i 9.

*Maze N.* Day 1. (a) 44; (b) 31; (c) 74; (d) 74; (e) 59. *Av.*, 56.4.

(b) Q. Does the old maze help you in this? A. The only way I can form an opinion of it is that it's just the same as the other one, only you form it in the opposite direction.

Day 2. (a) 210; (b) 76; (c) 138; (d) 1012; (e) 50. *Av.*, 297.2.

(a) V. No idea of it at all. I can't get my mind on it at all.

(d) S goes repeatedly as far as 5b and returns without making 6. He says repeatedly, "you can't get out except by going away from the center." After many minutes he is finally instructed to "feel *all* of *both* sides of *all* passages". At last he goes 5-6 by accident.

Day 3. (a) 115; (b) 30; (c) 108; (d) 43; (e) 30. *Av.*, 65.2.

Day 4. (a) 27; (b) 26; (c) 34; (d) 59; (e) 18. *Av.*, 32.8.

(b) Q. Try to remember next time how you remember how to go.

(c) While running course: V. I think that's where I have to go the other way [5-6]. Afterward: V. It's very much the same as if I saw the way it looked. I knew where I had made a mistake before and had to turn back toward the center.

(e) V. I got out quicker than I thought I would. [Motor?]

Day 5. (a) 14; (b) 22; (c) 12; (d) 13; (e) 11. *Av.*, 14.4.

(a) Q. Did you know how to go? A. Yes sir. Q. A general opinion [=visual imagery; see above]? A. No. Q. Did you think much about it? A. No, sir. Went right through.

(c) V. I knew exactly where to go. Didn't have to bother over it.—Apparently an abrupt change to kinesthetic consciousness, perhaps automatism.

Day 6. (a) 11; (b) 10; (c) 11; (d) 25; (e) 14. *Av.*, 14.2.

(d) S goes wrong, 6-6b; returns 6b-6-5, and then back 5-6-7 and out. V. I was miscalculating inside route. Q. How did you get out then? A. I knew where to come back to and get started over.—It seems that S returned to 5 to get a 'running start' to take him out, a characteristic of a motor habit.

Day 7. (a) 15; (b) 9; (c) 21; (d) 16; (e) 8. *Av.*, 13.8.

(e) Q. Did you think about it at each turn? A. I knew how to go. Q. You know how to tie your shoe, but you can do it without thinking much about it. Was it like tying your shoe? A. No, I don't think so, I had it on my mind all the time. I could tell in imagination how to go each time.

Day 8. (a) 15; (b) 9; (c) 11; (d) 12; (e) 9. *Av.*, 11.2.

(e) Q. How do you remember it? A. I have it in my mind where the gaps are. Q. When you get through one do you always think that you've gone through? A. Yes, sir; and I know where to get the next one.—Certainly the performance is not automatic, although, since S no longer has a 'general opinion', it may be kinesthetic.

Day 9. (a) 19; (b) 17; (c) 8; (d) 10; (e) 10. *Av.*, 12.8.

Day 10. (a) 14; (b) 15; (c) 11; (d) 11; (e) 8. *Av.*, 11.8.

Day 11. (a) 12; (b) 9; (c) 33; (d) 7; (e) 8. *Av.*, 13.8.

S has given practically the same report for the last three days as for day 8.

(c) S has same trouble at 5-5b as on day 2.

Day 12. (a) 14; (b) 10; (c) 6; (d) 12; (e) 8. *Av.*, 10.0.

(e) Q. How did you do it? A. Just knew how to go through. Q. What do you mean? A. I don't think I could set down and mark with a pencil how to go perfectly. But somehow when I get in the passages it just seems I know how to go to get through. Q. Can you see the formation of the maze now in your mind? A. Yes, sir. Q. Do you or don't you think about that while you are going through? A. I do and I don't. I see it some, but then again I don't see exactly where all the doors are. As I go through the curvature [S moves hand as if to indicate the turns of the maze] gives me some idea of how to go.—S is able to draw the maze now with three important mistakes. He is not certain how correct it is. Apparently he is running the course, dependent chiefly on kinesthetic cues, even if visual imagery is present.

Maze M is done by subject B apparently in visual terms throughout, although there is some intimation of a motor factor toward the end (tenth day). He recognizes maze N as the reverse of maze M, but has great difficulty with it. On the fourth day he is given the *Aufgabe* to introspect and reports visual imagery. In the last trial, however, a motor factor seems to appear, and the following day the performance is definitely kinesthetic. There is no indication of its becoming automatic up to the end. Some visual imagery appears to be present throughout,

but this seems not to be essential for the determination of the course. It may partially carry the *Aufgabe*.

### Subject C

Maze M. Day 1. (a) v 35; (b) v 14; (c) v 12; (d) v 10; (e) i 187.

Day 2. (a) v 9; (b) v 8; (c) v 7; (d) v 6; (e) i 141.

Day 3. (a) v 6; (b) v 6; (c) v 5; (d) i 19; (e) i 197.

(d) Q. How did you know how to get out? A. I could tell which way to turn generally. Very near knew where the doors were. Could tell by whether my hand was near center whether I had gone too far.

Day 4. (a) v 5; (b) v 4; (c) v 4; (d) i 27; (e) i 27.

(d) Q. How do you get out? A. I know when I'm in the center. I can't force my way out; I think I think what the drawing looks like.

Day 5. (a) v 5; (b) v 6; (c) i 17; (d) i 43; (e) i 28.

Day 6. (a) v 4; (b) v 4; (c) i 68; (d) i 16; (e) i 12.

(c) S starts out rapidly, but cannot, at first, get beyond 2-3-4a. V. I dreamed last night that I was drawing it and thought I knew it.

Day 7. (a) v 5; (b) i 17; (c) i 26; (d) i 14; (e) i 14.

(e) Q. How do you do it? A. I know it in my mind. Q. Do you see a picture of it in your mind? A. No, I don't. I just keep thinking I want to get better.—Further questioning fails to distinguish between 'knowing it in mind' and 'seeing it in mind'.

Day 8. (a) v 4; (b) i 38; (c) i 20; (d) i 29; (e) i 31.

Day 9. (a) i 15; (b) i 36; (c) i 18; (d) i 14; (e) i 18.

(e) Q. How did you remember how to go? A. Seems like I've been so used to drawing it I see it right in front of me. No, of course I don't see it, but I imagine it. One place I get mixed up. I know I'm wrong. Q. How do you know you're wrong? A. I don't know; I just can't find the gate. When I get in the long path [8] I know where I'm at. Q. How do you know you're right then? A. I just find I can go a long ways when I'm in it and I know I'm right. Q. Do you generally see the next gate in your mind before you get to it? A. No. Q. After you get to it? A. Yes, I see it then.

Day 10. (a) i 18; (b) i 19; (c) i 19; (d) i 15; (e) i 15.

(a) V. I know it most all from the beginning. Q. How do you remember it? A. I start in the center and go like that.—S draws with finger on table, accompanying drawing with words, "up, down, up, down". Q. Do you see the course in your mind? A. Seems like I sort of see it.

(e) S is instructed to draw course on a separate sheet. Starts twice and gets confused. Then: V. Seems like I could do it better with my eyes closed.—S draws it readily third time behind the curtain making errors in 0-1-2-3, but not in the rest. Q. Why is it you draw it better with your eyes closed when you think the way it looks is what helps you most to get out? A. I don't know. I do sort of see it. Q. Did you have a picture of it in your mind when you drew this [the reproduction of the course]? A. Yes, sir. I sort of saw it. It's that way when I'm reading. I see things if I shut my eyes.

Day 11. (a) i 15; (b) i 14; (c) i 17; (d) i 14; (e) i 17.

Day 12. (a) i 14; (b) i 13; (c) i 13; (d) i 13; (e) i 15.

(e) Q. How did you remember how to get out? A. Seems like I knew it was perfect. I draw right along. Q. What is in your mind when you're drawing right along? A. I'm thinking about my pencil and moving; and sometimes I get mixed up in the doors there. Q. Do you think about the way the passages look? A. Yes, sir. Q. Of the way it feels to go round? A. Yes, sir; it's more by the way it feels.

*Maze N.* Day 1. (a) 224; (b) 457; (c) 105; (d) 53; (e) 71. *Av.*, 182.0.

Day 2. (a) 36; (b) 49; (c) 44; (d) 30; (e) 29. *Av.*, 37.6.

(a) Q. Do you remember how to go? A. Yes. After I get to one door on the left, I seem to know where I'm at. Q. Is it a long ways out after that door? A. Yes, I have to make several turns. I come right out too.—Perhaps the door is 2-3.

(e) Q. How did you remember how to get out? A. Seems like you just keep your pencil going like. It doesn't pay to stop.

Day 3. (a) 48; (b) 28; (c) 22; (d) 23; (e) 21. *Av.* 32.4.

(b) How did you remember it? A. It sort of comes to my mind when I get to each place. I kind of know how to go.

(c) Q. Was that like last time? A. Yes. It seems like the more my pencil goes through one passage, the more I know where I'm at the next time. Q. How do you know where you're at?—S is puzzled. Finally: A. Don't know; I just sort of feel with my hand where the next door is going to be.—Apparently the performance is already kinesthetic.

(d) V. I just keep on moving my hand about and it seems like I get out.

(e) Q. Is that the same? A. Yes, sir. Only when my hand stops going, then I think of it. It doesn't pay to stop. Then I run over the same line. Q. Why do you go over the same line? A. The stopping mixes me up.

Day 4. (a) 69; (b) 26; (c) 28; (d) 20; (e) 17. *Av.*, 32.0.

(e) Q. When you go the right way in a turn, do you think how to do it? A. No. Q. Do you do it without thinking about it then? A. No, not that either. It seems like my hand was just going with the pencil. Sometimes when I go wrong I think more about it.

Day 5. (a) 27; (b) 26; (c) 17; (d) 19; (e) 16. *Av.*, 21.2.

Day 6. (a) 20; (b) 23; (c) 17; (d) 17; (e) 15. *Av.*, 18.4.

(a) V. I remembered it pretty well at that time. It seemed like the way my hand was going round, I knew pretty well where I was at.

Day 7. (a) 15; (b) 14; (c) 14; (d) 12; (e) 18. *Av.*, 14.6.

(b) V. I'm a little bit puzzled toward the end. The first part is the way my hand goes. It seems like I have to think more about it when I'm puzzled.

Day 8. (a) 17; (b) 14; (c) 12; (d) 11; (e) 8. *Av.*, 12.4.

(e) V. You know the door I've been missing? [S has been going 5-5b-6.] I've just discovered, if I keep my pencil up to the side, I'd go right on through. Q. What did you think about in the maze? A. Don't remember anything, except at that door.—Apparently the process is now automatic, except at one or two difficult points.

Day 9. (a) 10; (b) 9; (c) 8; (d) 6; (e) 6. *Av.*, 7.8.

(a) Q. How do you remember it? A. I guess by going over it so often. Q. That's *why* you remember it. *How* do you do it? A. I don't know. Q. What do you think about while you go through? A. I don't think about anything, sir [answer positive].

(e) Q. Do you think much about the maze? A. Well, I know I'm drawing it. Q. Do you think which way to go as you go through it? A. No, sir; no, sir. [Shakes head emphatically.]

Day 10. (a) 9; (b) 7; (c) 6; (d) 8; (e) 7. *Av.*, 7.4.

S answers questions in the same way and as positively as on preceding day. He draws maze with open eyes on a separate sheet, making three errors. Seemingly the process is now almost entirely automatic or at least unclearly kinesthetic.

(e) Q. Did you ever think this maze was like the last one? A. No, sir; it is nothing like the last one. [Positive.]

Subject C appears to be guided throughout maze M by visual imagery, although on the tenth day he finds that visual sensory elements added to the imaginal ones tend only to confuse him. The visual imagery, however, seems to remain important. With

maze N the kinesthetic factor appears as early as the second day and is very important on the third day and thereafter. The performance has become largely automatic by the eighth day and almost completely so on the ninth day. Visual imagery seems to play very little part in the performance with maze N. It is probably present early and in difficult parts later, just as the kinesthetic elements appear at difficult points after the rest of the performance has become automatic.

*Subject D.*

*Maze M.* Day 1. (a) v 55; (b) v 35; (c) v 12; (d) 18; (e) i 543.

Day 2. (a) v 9; (b) v 19; (c) v 20 (d) v 10; (e) i 685.

S is mute and has to be urged constantly to keep at the task.

Day 3. (a) v 14; (b) v 7; (c) 13; (d) i 448; (e) i 555.

Day 4. (a) v 8; (b) v 10; (c) v 8; (d) i 440; (e) i 60.

Day 5. (a) v 12; (b) v 6; (c) i 76; (d) i 42; (e) i 32.

Day 6. (a) v 13; (b) i 25; (c) i 57; (d) i 24; (e) i 37.

S is still mute, although he sometimes answers questions by nods. He co-operates otherwise fairly well.

Day 7. (a) v 7; (b) i 25; (c) i 46; (d) i 19; (e) i 22.

(e) Q. How do you remember how to go? (This question has often been asked before, but S has never answered it.) A. Feel it, I think. I kind of remember which way I went before. Q. Do you keep thinking about the way the maze looks? A. Well, yes; I do in a kind of indefinite way. But I think principally about the feel.

Day 8. (a) v 7; (b) i 17; (c) i 14; (d) i 18; (e) i 14.

Day 9. (a) i 16; (b) i 15; (c) i 17; (d) i 12; (e) i 12.

(e) Q. How do you remember how to go? A. I feel the way I've been before. Seems to come naturally to me. That's about all. Q. Do you keep seeing what the maze looks like or is it all just feeling? A. It's all just feeling; I feel with my pencil.

Day 10. (a) i 23; (b) i 56; (c) i 16; (d) i 14; (e) i 14.

Day 11. (a) i 21; (b) i 15; (c) i 15; (d) i 20; (e) i 20.

S has been mute the last two days.

Day 12. (a) i 20; (b) i 13; (c) i 10; (d) i 9; (e) i 13.

(e) Q. How did you remember how to get around? A. It just came natural to me. I've been around so much. Q. Did you have to think about it all the time. A. Yes [positive]. Q. Did you think about the way it looked or the way it felt to do it?

A. It was mostly by the feel.—S is asked to draw the course on a separate sheet, but hesitates very much and finally gives up after placing a few right angles on the sheet. It would seem that the 'feel' is more important than visual imagery.

Maze N. Day 1. (a) 244; (b) 133; (c) 176; (d) 56; (e) 43. *Av.*, 146.4.

(d) Q. Did you remember how to get out? A. Kind of; yes. I had a kind of indefinite feeling, just the same as on the other maze.

Day 2. (a) 107; (b) 33; (c) 24; (d) 299; (e) 51. *Av.*, 102.8.

(b) Q. How did you know how to get out? A. By the sound of the pencil. Q. How did that help you? A. I don't know. There was a kind of indefinite rhythm about it. It played a tune.

Day 3. (a) 69; (b) 23; (c) 24; (d) 36; (e) 40. *Av.*, 38.4.

(c) V. That was the best I ever done, because I put my mind on it. Q. What did you find when you put your mind on it? A. Found it easier to get out. Q. Did you know where you were in the maze? A. I just felt with my mind. Q. Can you tell me how that was? A. I kind of anticipated where I was going to go through the next door.

Day 4. (a) 182; (b) 32; (c) 60; (d) 51; (e) 29. *Av.*, 70.8. Makes error first time of going into 5b and then back to O, and continues it in all trials. S is almost mute today.

Day 5. (a) 81; (b) 21; (c) 22; (d) 15; (e) 16. *Av.*, 31.0.

(b) Q. What was the difference between this time and last time? A. Lots! I didn't know what I was doing this time; last time I did.—A sudden lapse into automatism?

(d) Q. How was it that time? A. I got into the game that time. It's like football when you've got the ball. You want to get through and you just get through.—The *Aufgabe* is conscious, the performance less so, perhaps automatic.

Day 6. (a) 31; (b) 24; (c) 18; (d) 12; (e) 18. *Av.*, 21.0.

(d) Q. Can you tell me how you got out that time? S thinks a while. A. No, I can't. Q. Did you think how the maze looked? A. No, I didn't. Q. Did you think about it at all? A. Yes, I tried to get out as hard as I could. Q. Anything else? A. No, I don't remember any.

Day 7. (a) 34; (b) 17; (c) 12; (d) 13; (e) 16. *Av.*, 18.4.

(e) Q. How was it today? A. I just thought about going. I went in that direction mostly [motions to left]. Q. Did you imagine the way the maze looked? A. No, I didn't.

Day 8. (a) 11; (b) 11; (c) 22; (d) 17; (e) 13. *Av.*, 14.8.

(e) Q. Did you get through by the feel or by thinking how the maze must look? A. By the feel. Q. Entirely, or do you think of the looks of the maze a little? A. Entirely. Q. Are you sure? A. Yes, I'm sure.

Day 9. (a) 19; (b) 16; (c) 8; (d) 16; (e) 14. *Av.*, 14.6.

Day 10. (a) 14; (b) 8; (c) 11; (d) 14; (e) 10. *Av.*, 11.4.

(e) Q. How did you remember the maze these times when you went so fast? A. Why I didn't think about anything, except about getting out. Q. When you thought about getting out, did you think about the end of the maze or did you think of getting by each place?—S thinks for a long time. A. I didn't think about the end of the maze or about each place either; I just thought about getting out. I thought I must hurry.—Apparently only the *Aufgabe* remains conscious.

Day 11. (a) 16; (b) 13; (c) 8; (d) 17; (e) 10. *Av.*, 12.8.

Day 12. (a) 12; (b) 13; (c) 12; (d) 15; (e) 14. *Av.*, 13.2.

(e) Q. How did you remember it? A. I didn't remember anything. Q. How did you get through then? A. Oh, I remember it some by the feel. That maze, there's a give to it,—a kind of give where the openings are. I remember the way it gives, the way it feels when you push against the openings.—S's description corresponds well to the clear kinesthetic experience that the writer gets, when with one motion the pencil can be made to shoot through three or four doors. S is asked to draw maze and draws a continuous course, that resembles the actual one but little, except at the last (6-7-8-x).

On account of the muteness of D, the introspections for maze M are incomplete. He seems, however, to have followed a kinesthetic cue. With maze N, the kinesthetic factor is important from the start. Visual imagery is not reported at all. There is evidence of the performance becoming automatic about the fifth day, but there is also evidence that the kinesthetic factor had not entirely vanished at the end of the series. The *Aufgabe* to get out quickly appears to have remained conscious throughout.

### Subject E

Maze M. Day 1. (a) v 18; (b) v 12; (c) v 10; (d) v 8; (e) i 23.

(e) Q. Did you know where you were? A. I knew a good deal of the time, at the start and the end.

Day 2. (a) v 8; (b) v (6); (c) v 6; (d) v 6; (e) i 53.

Day 3. (a) v 7; (b) v 6; (c) v 5; (d) i 280; (e) i 163.

(e) V. I try to go back to the center to get the trail. I feel as if I could get out if I started right.

Day 4. (a) v 6; (b) v 5; (c) v 4; (d) i 677; (e) i 21.

(d) S at first cannot get beyond 3-4a, and then beyond 5-6-7a.

Day 5. (a) v 5; (b) v 5; (c) i 47; (d) i 167; (e) i 33.

Day 6. (a) v 5; (b) v 5; (c) i 51; (d) i 47; (e) i 15.

(c) Q. How do you know how to get through? A. I have an idea how the maze is formed. I think a good deal of the way it looks; I think where the doors ought to be and feel for them.

Day 7. (a) v 4; (b) i 16; (c) i 17; (d) i 9; (e) i 10.

Day 8. (a) v 5; (b) i 10; (c) i 10; (d) i 12; (e) i 8.

(e) Q. Tell me all you can about how you remembered it. A. I remembered about how many times I had to go round. To keep off the wrong alleys you have to judge about how far to go. As soon as I felt I was moving my hand too far, I felt I was wrong and went back. As much as I could I tried to picture the way it looked when I'd seen it. Q. Did you see it pretty clearly? A. Yes.

Day 9. (a) i 9; (b) i 8; (c) i 11; (d) i 8; (e) i 8.

(e) Q. How do you remember it? A. A good deal from seeing it. You have to get the movements formed in your mind before you start out. Then you go around and feel part of the way. Q. How much do you do by feel and how much by the way it looks? A. About one-half by memory,—forming it in your mind the way it is,—one-quarter by feeling, and one-quarter by the looks of it when you last saw it. Q. What do you mean by "forming it in your mind the way it is"? A. The way it looked to go right. That's different from the other; the memory of the looks of the maze has the wrong ways to go in it, too.

Day 10. (a) i 10; (b) i 7; (c) i 16; (d) i 17; (e) i 5.

(e) Q. How was that? A. Did a lot by feeling, more than by memory. Q. Did you think about each gate? A. No; I thought it would pay to rub the sides and feel the way, as well as to have in my mind [=visual imagery of the right course; see above] about where the gates are.

Day 11. (a) i 7; (b) i 8; (c) i 6; (d) i 6; (e) i 5.

Day 12. (a) i 6; (b) i 7; (c) i 7; (d) i 5; (e) i 5.

(e) Q. How do you remember to get through? A. Memory [=visual imagery] about two-thirds; then also by feeling and the way it looked last time I saw it. [Cf. day 9.] Q. What do you mean by memory? A. Memory of the door you go through,—

according to looks.—S now draws maze on separate sheet perfectly, accompanying it with verbal comment, *e.g.*, "First you go up, then you go down," etc. Evidently the memory is largely visual, but little kinesthetic.

*Maze N.* Day 1. (a) 780; (b) 140; (c) 45; (d) 40; (e) 17. *Av.*, 204.4.

(e) Q. How did you do that? A. I did it a little bit by memory, like last time. I could tell by actions, too, how it felt, just where I was. By knowing where the door is I get the lay of the land. If it were all turned around [indicates turn through 90°] I couldn't do it as easy. But by its always being one way, I get the feel of the actions, just where each gate is.

Day 2. (a) 22; (b) 16; (c) 14; (d) 14; (e) 12. *Av.*, 14.6.

Before beginning S presents a drawing of the right course which he has made at his leisure in the ward. It has one error, in that it omits 6 in 5-6-7.

(a) S fails to find 5-6 at once, the error in his drawing. V. One place there kind of slipped my memory.

(b) V. I did that by memory and by feeling. Q. When do you use feeling? A. I use feeling when I think I'm a little lost from my memory.—S seems to try to replace the kinesthetic cues of last time by visual, as far as possible.

Day 3. (a) 10; (b) 9; (c) 7; (d) 7; (e) 7. *Av.*, 8.0.

Day 4. (a) 7; (b) 8; (c) 7; (d) 6; (e) 7. *Av.*, 7.0.

(a) Q. How did you remember it? A. At the start I paused to think which way I ought to go. Q. Did you picture it in your mind? A. Yes; if I do that I don't get lost so easy. When I started I happened to think, "Which way do I make the first turn?" Thought right away it was towards me.—S describes course throughout.

(e) V. As soon as I've gone through a door I can tell by the feel pretty much how to go.—Apparently S has some motor cues, although he reports visual in most cases.

Day 5. (a) 7; (b) 7; (c) 7; (d) 7; (e) 6. *Av.*, 6.8.

(b) Q. How are you doing it? A. I imagine a picture of the maze. I see the way all the time. When I get on a street I know just which way to go and find my way out.—The behavior of S bears this statement out. He pauses slightly before each door and feels for it. He does not make the long sweeps through several doors, that seem to come with the motor habit.

(e) V. Here's the thing that bothers me [indicates, by drawing on the table, 5-6-7-8]. I don't seem to touch anything. I hit the door too easy.—In a purely motor habit 5-6-7-8 is made by a

single sweep of the arm. That the ease with which this may occur disconcerts S, is an indication that the performance is not motor.

Day 6. (a) 6; (b) 5; (c) 5; (d) 5; (e) 4. *Av.*, 5.0.

(c) Q. Do you definitely think of how to go at each turn? A. Yes. I see the drawing of the maze. Q. Does the feel of the maze help you? A. About one-fifth. I don't feel as much for the openings as at first, because I've got so now I know pretty well where they are. Q. How do you know where they are? A. By the plan. I see in my mind where they are.—S has almost completely substituted visual imagery for kinesthesia.

(d) V. I went so fast that my mind could hardly keep up with my hand. It bothered me. I'm going to try to do it next time by feeling more and not to have to think as fast as I go.

(e) V. I done that part by feeling. I went too 'cautionate' by thinking and knowing where I was. By feeling you don't go so 'cautionate' but the sides keep you where you ought to be at.—Thus S consciously adopts a kinesthetic guide and saves a second.

Day 7. (a) 6; (b) 9; (c) 6; (d) 5; (e) 4. *Av.*, 6.0.

(b) S makes error in 5b. Q. What was the matter? A. I let my hand go too much and got in the wrong place.

(e) Q. How did you do it that time? A. A good deal by memory of how it looked and partly by the way it felt.—Visual imagery again.

Day 8. (a) 7; (b) 5; (c) 5; (d) 4; (e) 4. *Av.*, 5.0.

(b) Q. How was that? A. I was thinking about it. I didn't like to trust to the way it felt.—So S discards the kinesthetic guide and returns to the visual as more trustworthy.

(e) S is asked to draw maze and does so perfectly and with confidence. Q. Did you think this maze was anything like the last one? A. I thought it was somewhat similar. I don't think it's like it.

Subject E is evidently strongly visual. Maze M is learned entirely in visual terms, and does not become automatic. Maze N, lacking visual perceptual cues, is remembered in the first five trials somewhat kinesthetically. The subject then, however, draws out the course for himself on paper, and thereafter visual imagery plays the important rôle, to the partial exclusion of kinesthesia. Later (sixth day) the subject consciously discards the visual for the kinesthetic performance, because he finds it difficult to keep the visual imagery clear while going rapidly; but

he discovers that kinesthesia is not immediately reliable and returns (probably seventh day) to the visual method.

*Subject F*

*Maze M.* Day 1. (a) v 28; (b) v 12; (c) v 8; (d) v 7; (e) i 159.

Day 2. (a) v 8; (b) v 8; (c) v 5; (d) v 5; (e) i 48.

Day 3. (a) v 6; (b) v 6; (c) v 5; (d) i 37; (e) i 179.

Day 4. (a) v 5; (b) v 4; (c) v 5; (d) i 73; (e) i 27.

(e) Q. How do you remember it? A. I know where the openings are.—S is asked to draw maze, but does not succeed at all. Perhaps his performance is not visual. If motor, it is far from the perfection that is apt to occur before the maze habit becomes motor, for S makes many errors.

Day 5. (a) v 4; (b) v 5; (c) i 15; (d) i 13; (e) i 13.

S is questioned repeatedly about his manner of performance, but is unable to give significant answers.

Day 6. (a) v 4; (b) v 4; (c) i 19; (d) i 12; (e) i 14.

Day 7. (a) v 4; (b) i 14; (c) i 12; (d) i 9; (e) i 14.

(d) Q. How do you know how to go? A. I know the way it goes. I tell by the feel. [Marks out imaginary course with finger in the air.]

Day 8. (a) v 4; (b) i 14; (c) i 7; (d) i 12; (e) i 6.

(e) Q. How do you know how to go? A. I go by the feeling. It goes right around. I can tell ahead of me where I'm going. Once in a while I get lost.

Day 9. (a) i 9; (b) i 19; (c) i 7; (d) i 7; (e) i 6.

(e) Q. How do you remember how to go? A. Just by going around. Q. What are you thinking of while you're going around? A. I can tell if there's anything in front of me or behind me. There's a feeling comes over me. I can tell when harm is coming before it get's inside of four feet, front or back. Q. Where is the feeling? A. It strikes me here about [indicates chest and sides]. I get the same feeling when I'm running the maze. Q. Is the feeling the only thing that tells you how to go? A. That's the only thing. Nothing else.—S declares that he always has this feeling for anything that he cannot see.

Day 10. (a) i 7; (b) i 5; (c) i 10; (d) i 15; (e) i 16.

(a) S is told before trial to observe what he thinks about while running the maze. After trial: V. That time I just felt my way. Q. Did you know how you were going? A. No; just kind of kept going till I found my way out [motions rhythmically with pencil in air].

(e) In last three trials S has been confused in making 4-5. Heretofore he has always made 3-4-5 in one sweep. This time he overcomes the difficulty in making 4-5 by returning to 3 again and making 3-4-5 in one movement as before, remarking at the time, "I have to go back and get a new start". He always makes 6-7-8 and sometimes 5-6-7-8 in one sweep of the arm. The getting of the new start indicates that the consciousness may be a sort of a rhythmical consciousness, and even that it may involve the organic factors usual to a rhythmical consciousness (see day 9).

Day 11. (a) i 10; (b) i 6; (c) i 5; (d) i 4; (e) i 11.

Day 12. (a) i 7; (b) i 9; (c) i 5; (d) i 4; (e) i 3.

(b) Q. How did you do it? A. I was feeling my way out.—Longer time in this trial and preceding is due to the fact that in attempting 5-6-7-8 he stops short of necessary distance for getting into 8, and comes back into 7b. Each time he corrects himself by returning to 5 and making 5-6-7-8 in one sweep. Evidently this is a single unit in a motor chain.

(e) Makes one full error and several false starts even at this speed. Is unable to draw maze at all correctly.

Maze N. Day 1. (a) 372; (b) 162; (c) 85; (d) 27; (e) 35. *Av.*, 136.2.

Day 2. (e) 34; (b) 42; (c) 63; (d) 15; (e) 14. *Av.*, 33.6.

(a) Q. How do you remember the thing? A. By the way it goes. Q. Do you mean the looks? A. No, by the way it goes. Q. What do you mean by that? A. I remember just how it goes [motions with arm and shoulder in air].

Day 3. (a) 17; (b) 8; (c) 12; (d) 11; (e) 20. *Av.*, 13.6.

(a) Q. How do you do it? A. Just keep looking for openings so I can get out. If I can't find an opening I go right on.—This statement well describes the actual behavior of S. He moves rapidly all the time, frequently breaking pencil points. When confused he may move rapidly back and forth over not more than an inch of path, and he continues violently bumping around until he gets out.

Day 4. (a) 9; (b) 6; (c) 19; (d) 13; (e) 9. *Av.*, 11.2.

(b) Q. Do you know how to go? A. No, I just get in and keep going around. The faster I go the quicker I get out, so long as I don't hesitate. As soon as I go to hesitation, it takes me a long while to get out.—S has in some instances jumped the walls in his haste. On being put back he is always confused and it takes him some time to get again what is apparently his motor adjustment.

Day 5. (a) 9; (b) 5; (c) 11; (d) 14; (e) 7. *Av.*, 9.2.

(b) V. I have to go fast to get through. If I go slow I get all mixed up. I have to hurry and then I make mistakes.

In general S makes 0-1-2, 3-4-5, and 5-6-7-8, each as a single movement. His troubles are of two kinds: either he wanders back and forth in a bewildered manner before he can connect two of these units, or his adjustment falls short so that he misses the last door in one and finds himself almost hopelessly lost.

Day 6. (a) 7; (b) 5; (c) 8; (d) 7; (e) 7. *Av.*, 6.8.

(d) V. I don't think much about it. I just keep going right through.

Day 7. (a) 10; (b) 6; (c) 8; (d) 4; (e) 11. *Av.*, 7.8.

Day 8. (a) 9; (b) 10; (c) 6; (d) 7; (e) 6. *Av.*, 7.6.

(a) Q. How did you do it that time? A. When I first start there are two or three little places where I can't go; then just as soon as I can get out of them I can go the full length. Q. After you get out of them do you have to think about your going? A. No, I go right along. It just takes one whole draw [moves arm about in an easy sweep].

Day 9. (a) 7; (b) 7; (c) 6; (d) 6; (e) 5. *Av.*, 6.2.

S still makes many errors, as many as six in a single trial, but moves so rapidly that he gets out in short time in spite of them. He is unable to draw the maze satisfactorily on a separate sheet.

Subject F appears to be as strongly motor as E was visual. He reports no visual imagery at any time, but learns both mazes principally in motor terms. Although he makes very short times, the performance remains far from perfect. Apparently he knows the maze in three or four units, each of which he can shoot through rapidly if that particular performance is touched off properly. His course in both mazes resembled a violent bumping around with occasional spurts through several passages, as he got the right cue for that particular unit. It is possible that for this subject these units group themselves into a sort of rhythmical consciousness, which may be organically carried. It is not possible to say whether the performance ever became automatic or remained purely kinesthetic.

#### *Subject G*

*Maze M.* Day 1. (a) v 28; (b) v 14; (c) v 10; (d) v 10; (e) i 129.

Day 2. (a) v 10; (b) v 8; (c) v 6; (d) v 6; (e) i 142.

Day 3. (a) v 7; (b) v 6; (c) v 6; (d) i 72; (e) i 78.

Day 4. (a) v 7; (b) v 6; (c) v 5; (d) i 148; (e) i 29.

(d) V. I can tell when I'm on the right track from being familiar with the course. Q. How can you tell? A. I see the way in my mind.

Day 5. (a) v 6; (b) v 5; (c) i 37; (d) i 26; (e) i 22.

Day 6. (a) v 5; (b) v 4; (c) i 33; (e) i 25; (e) i 33.

(c) V. I know I have to follow a little circle, then go to here [motions in air], then two breaks and through. It's just like a fireplace [closes hand and diagrams in air]. You have to get out of the fireplace and then around the mantel.—Evidently this is visual imagery.

Day 7. (a) v 6; (b) i 23; (c) i 17; (d) i 21; (e) i 30.

Day 8. (a) v 5; (b) i 15; (c) i 34; (d) i 37; (e) i 37.

(e) Q. How do you remember it? A. I know I have to go this way. [Traces course with closed eyes on table.] There's a notch, here's a door, then there's a street, etc.—Evidently S is still using visual imagery.

Day 9. (a) i 23; (b) i 20; (c) i 17; (d) i 23; (e) i 15.

(e) Q. How do you remember how to go? A. I remember the formation. It's like an oriental design,—a grill-work.—S is asked to draw maze with eyes shut on separate paper. He does so with only one important error, although the lines cross each other very often. He draws slowly, pausing as if to think, suggesting reliance upon visual imagery.

Day 10. (a) i 23; (b) i 15; (c) i 18; (d) i 16; (e) i 14.

(a) V. I know the formation now. We call that casaban effect on a lace curtain. [S has been dry-goods salesman.]

Day 11. (a) i 84; (b) i 22; (c) i 22; (d) i 22; (e) i 21.

(e) Q. How do you do it? A. I do it by remembering the formation, the way it looks. I can draw a sketch of it.—S is asked to draw the course, eyes open. His drawing is correct for all the turns, although poorly proportioned.

*Maze N.* Day 1. (a) 214; (b) 129; (c) 71; (d) 66; (e) 286. *Av.*, 15.2.

Day 2. (a) 152; (b) 62; (c) 135; (d) 93; (e) 457. *Av.*, 179.8.

(e) Goes back and forth in 2, 3, and 4, but does not attempt 4-5 for a long while. When S does make 4-5, he gets right on through.

Day 3. (a) 41; (b) 40; (c) 31; (d) 35; (e) 23. *Av.*, 34.0.

(a) Q. Do you or do you not think of the way the maze looks? A. Oh, I see it somehow. I try to get the thing in my eye. That's the way I learn things.

(c) V. When I get out from the starting point, I know just where I am. Q. Is it by the way you remember the maze that you know where you are? A. Yes, and by the sense of touch. Over at the right I get in a little pocket and feel my way out by the sense of touch. And when I'm coming down the long passage [probably 8] I feel I'm going to get out.

(e) Q. How much of that was touch and how much eye? A. Greater portion eye. I went through by touch and the eye and the brain traveled with the pencil. I know when I'm wrong, as I see it with my eye, as it were.—Evidently still visual imagery with some kinesthesia is S's method.

Day 4. (a) 32; (b) 25; (c) 19; (d) 35; (e) 19. *Av.*, 26.0.

(e) V. It brings two senses together, the sense of feel and the sense of thought. The thought comes when I am wrong. There are a number of places on which I'm uncertain. Then the thought comes in. The sense of feel is when I'm all right. I know how to strike in against the walls.—This statement, volunteered entirely without suggestion from the experimenter, may be taken to mean that kinesthesia has already entered in to a large extent and that visual imagery is important only in difficult places.

Day 5. (a) 18; (b) 22; (c) 19; (d) 16; (e) 14. *Av.*, 17.8.

(e) V. I knew it when I started out. I had my mind on the diagram. But I went by the door. I made too much haste. [Error at 4-5.]

Day 6. (a) 15; (b) 22; (c) 13; (d) 16; (e) 15. *Av.*, 16.2.

(a) V. It goes all right for a while; then when I get here [indicates 5-6-7-8 by drawing on the table] it's sort of a mystery how I get through. It's sort of a double ditch. I jump through like a spring. When I get through I know I'm all right, but it's a mystery how I get there.—The course 5-6-7-8 is the part which is made so easily in one sweep of the arm and which bothered subject E, who was proceeding with visual imagery. The experimenter also found the ease and rapidity with which the three doors could be passed very surprising indeed.

Day 7. (a) 10; (b) 14; (c) 11; (d) 11; (e) 17. *Av.*, 12.6.

(e) Q. How do you think of it? A. I see where all the little notches are—just as I think of anything you know. It's just as if I had a thing committed to memory.

Day 8. (a) 15; (b) 19; (c) 19; (d) 12; (e) 10. *Av.*, 15.0.

Day 9. (a) 16; (b) 11; (c) 11; (d) 9; (e) 8. *Av.*, 11.0.

Day 10. (a) 16; (b) 10; (c) 9; (d) 9; (e) 24. *Av.*, 13.6.

(e) Q. How did you do it? A. Give me that pad and I'll show

you.—S is given pad and draws route correctly in about 5 seconds.  
 Q. Does that mean that you see the thing in your mind that way when you do the maze? A. Yes, I see it all the time.

Q. Is this maze anything like the first? A. It's similar, only reversed.

Subject G, like E, is strongly visual and negotiates the difficulties of both mazes throughout in visual terms. In maze N, however, there are also at least some kinesthetic factors, mentioned especially on the third day, which probably continue throughout, secondary to the visual factors, which alone are reported later. Possibly the kinesthetic part becomes automatic at the end. The larger part of the complex, however, remains conscious as visual imagery.

### *Subject H*

*Maze M.* Day 1. (a) v 18; (b) v 20; (c) v 16; (d) v 10; (e) i 326.

Day 2. (a) v 15; (b) v 8; (c) v 8; (d) v 7; (e) i 83.

Day 3. (a) v 9; (b) v 8; (c) v 6; (d) i 62; (e) i 32.

Day 4. (a) v 9; (b) v 7; (c) v 5; (d) i 15; (e) i 13.

(d) V. I didn't think about the maze. I was counting while I was doing it. I felt as if my hand were just driving along.

(e) Q. How was it this time? A. Just struggling my way. Just struggling all along. I thought of nothing else. I got out myself this time.—S explains that last time he thought that some one else got out for him while he was thinking of something else. This is quite in keeping with S's delusions.

Day 5. (a) v 5; (b) v 6; (c) i 16; (d) i 12; (e) i 14.

Day 6. (a) v 6; (b) v 6; (c) i 12; (d) i 9; (e) i 15.

Day 7. (a) v 5; (b) i 14; (c) i 14; (d) i 25; (e) i 12.

(e) V. Just feel along. I know the pencil is bound to reach from one groove to another; I hold the pencil on the side of the maze from which I know it runs out. Hardest thing is to decide if I run out whether I am in right direction—whether I'm going out or back to the center. Q. How do you know which side the opening is on? A. I try to get out to the longer line and then I know I'm right. When I can't get out on one side I try the other.—Certainly this report does not indicate that the performance is surely motor, in spite of the short time; its exact character is uncertain, whether visual, verbal, attitudinal or motor.

Day 8. (a) v 6; (b) i 11; (c) i 14; (d) i 8; (e) i 7.

Day 9. (a) i 9; (b) i 11; (c) i 11; (d) i 17; (e) i 7.

Day 10. (a) i 13; (b) i 10; (c) i 12; (d) i 12; (e) i 26.

(e) Q. How do you remember how to go? A. I don't remember how to go. I go with the pencil. I follow the pencil and when it goes out I go with it. I just go along through the maze.—The performance is kinesthetic, perhaps automatic.

Day 11. (a) i 7; (b) i 7; (c) i 5; (d) i 9; (e) i 12.

Day 12. (a) i 4; (b) i 8; (c) i 6; (d) i 4; (e) i 7.

(d) Q. How do you feel when you go through the maze? A. Oh, I feel my hand just go through. It goes lightly. Most of the time I hardly realize I'm going through the maze.—S fails to draw the course at all correctly. Evidently kinesthesia and automatism account for his final performance.

*Maze N.* Day 1. (a) 218; (b) 31; (c) 68; (d) 24; (e) 22. *Av.*, 76.2.

Day 2. (a) 17; (b) 22; (c) 17; (d) 15; (e) 15. *Av.*, 17.2.

(a) Q. How did you get out then? A. I don't know. I don't know where the openings are. I just feel with the pencil.

(e) 4 errors. Q. How did you do that? A. I don't know. I have to work my mind to get out. I work my way out. I don't know where the passages are.

Day 3. (a) 13; (b) 10; (c) 9; (d) 8; (e) 10. *Av.*, 10.0.

(a) Q. How did you remember it? A. I don't remember it. I haven't seen it. I just imagine how it is and then go with the pencil. Q. Do you imagine how the maze must look or how you ought to move your pencil? A. I just imagine how it looks.

(d) Q. Do you see the maze all the time in imagination while you are going through? A. Yes. Q. Is that what makes it possible for you to go through? A. I don't think about it most of the time. I just see it before me. If I were to think about how I'm going, I would have to go very slowly. It just seems as if I went along unconsciously and got out. I have to go too fast to pay attention.—The performance is then almost automatic already. The visual imagery, though present, is not the guide by which the subject proceeds. Perhaps it carries the *Aufgabe*.

Day 4. (a) 14; (b) 11; (c) 6; (d) 8; (e) 6. *Av.*, 9.0.

(c) Q. Did you think every time you went one way just how to go? A. No. I see the whole maze in imagination all the time. But if you show it to me I think I would get mixed up and have to go slow. I depend on the chance of speed to get me through.

After day 4, S complains about the work and refuses to continue with it.

Subject H reports only kinesthetic processes and automatic performances definitely. Both mazes were run in short times by kinesthetic guides before many trials, and probably the performance became automatic later. There is indication of processes other than motor at the start, but the subject is not explicit.

As far as it is possible to express such results schematically, we have attempted to summarize the temporal courses of learning for the different subjects in Table XVI. The principal initial, middle, and final conscious concomitants of the learning performance are given in three columns, "v" and "k" standing for visual imagery and kinesthetic processes respectively, and "a" for movement without consciousness, that is to say, automatic movement. It must be remembered that the table shows only what was definitely reported. Many processes may have been present that were not elicited by questioning. The table moreover aims to show only those processes which stood for (*i.e.*, meant) the means of guidance within the passages. Other processes relevant to the maze are not considered.

It will be observed that generally in the course of consciousness during the learning of the maze there is first visual imagery which gives place to kinesthesia, which in turn gradually becomes less and less clear until complete automatism is reached.<sup>1</sup> Four subjects began thus with visual imagery; in one (G) the processes became partly kinesthetic; in one (B), completely kinesthetic; in one (A), completely kinesthetic and then partially lapsed; and one (C), completely kinesthetic and then completely lapsed, that is to say, the movement became automatic. In four subjects the processes were kinesthetic from the start. In one of these (F) they remained kinesthetic; in one (D), they partially lapsed; in one (H), they lapsed completely; while in one (E) they became visual.

<sup>1</sup> This is the typical course of human maze learning found by Day and the writer, *loc cit*, and again by the writer, *op. cit*.

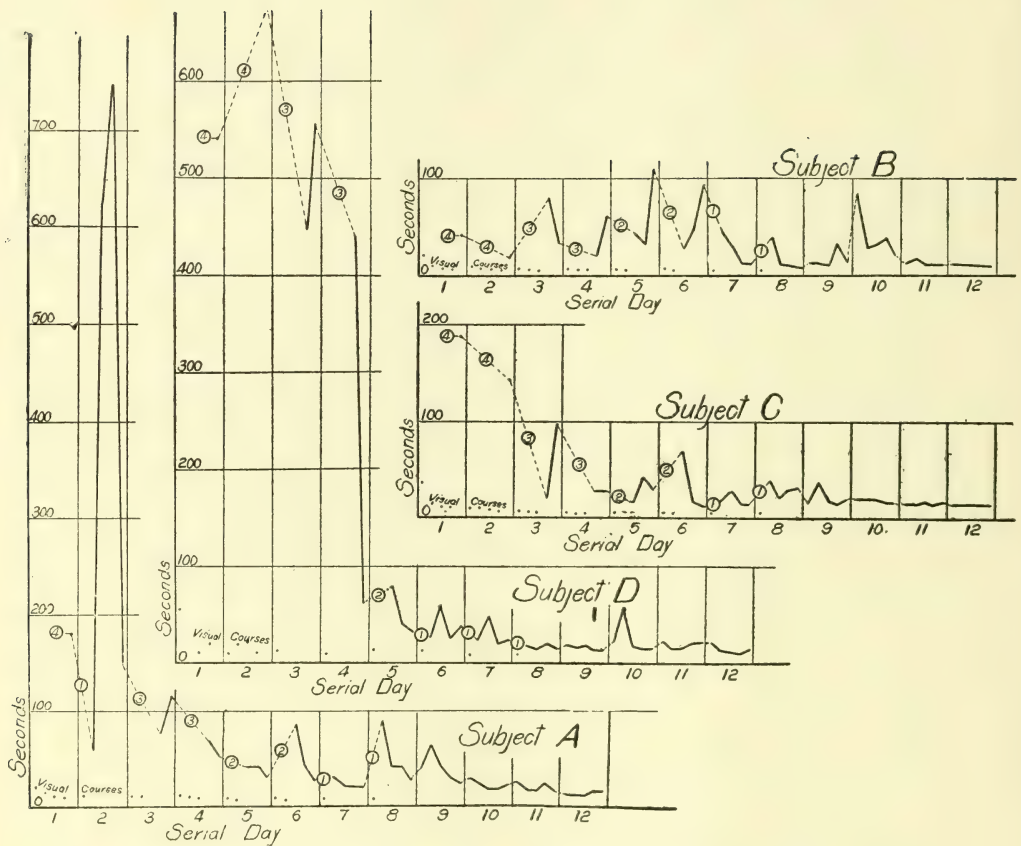
TABLE XVI

## CONSCIOUS PROCESSES

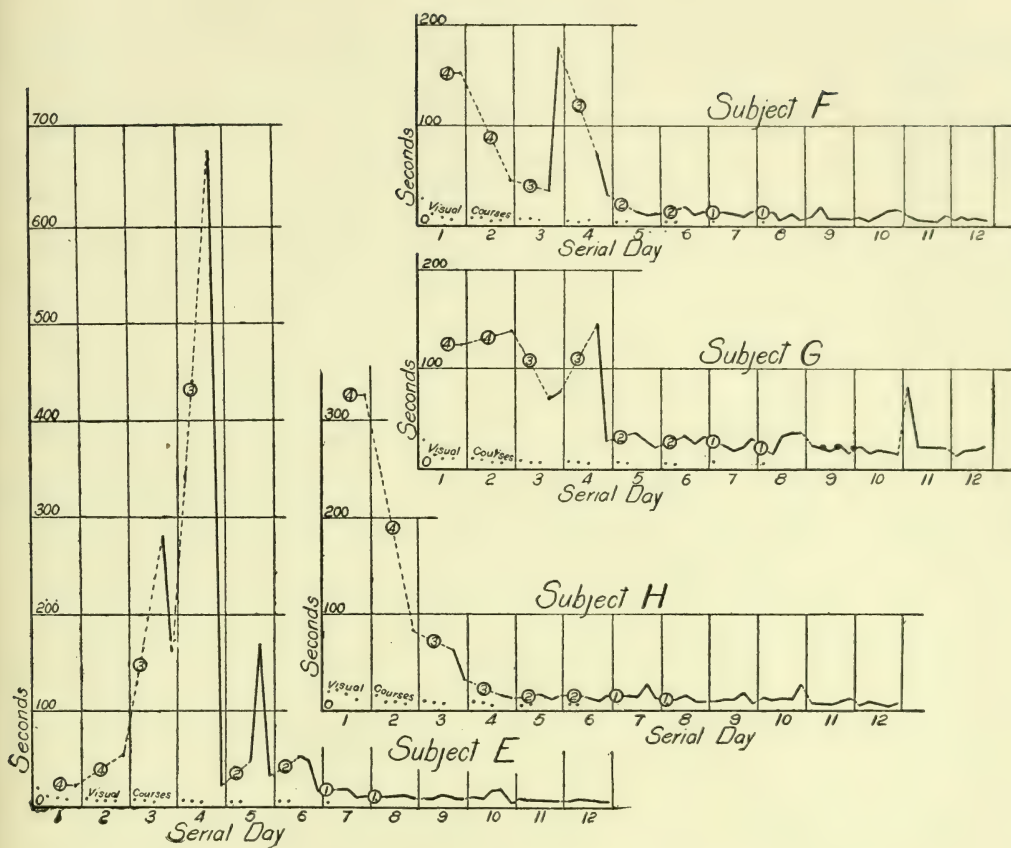
Representing the means of guidance in the learning of maze N.  $v$  = visual imagery;  $k$  = kinesthetic sensations or imagery;  $a$  = automatism (no conscious processes).

Subject	Conscious processes concomitant with:		
	Initial Performance	Middle Performance	Final Performance
A .....	$v$	$k$	$k + a$
B .....	$v$	$k$	$k$
C .....	$v$	$k$	$a$
D .....	$k$	$k + a$	$k + a$
E .....	$k$	$v + k$	$v$
F .....	$k$	$k$	$k$
G .....	$v$	$v + k$	$v + k$
H .....	$k$	$a$	

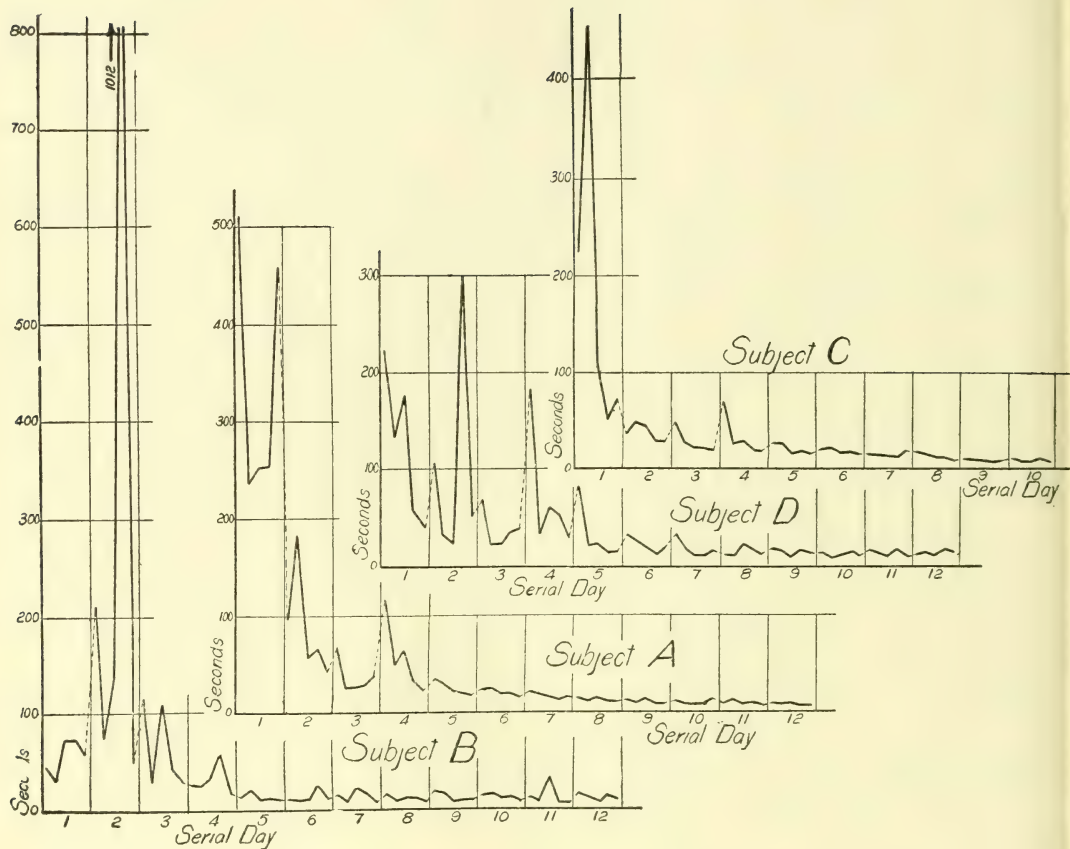
In order that the numerical relations may be better seen, the average times for maze N are tabulated in Table XVII. The subjects are ranked there upon their average times from day 2 to day 8 inclusive, day 1 being omitted because the times are determined largely by chance and their inclusion would do some subjects, who learn well, an injustice. The results are also shown quantitatively in curves. Figs. 18-25 show learning with maze M. The curve drawn in each case is for only those courses made with the maze obscured. The dotted lines indicate breaks between days, and the small numbers in circles upon the dotted lines, the number of trials made in the interval with the maze in sight. The actual values of the times of these visual courses are shown below the curves as unconnected dots. Figs. 26-33 are for maze N. Here there were no visual courses. The dotted lines indicate breaks between days.



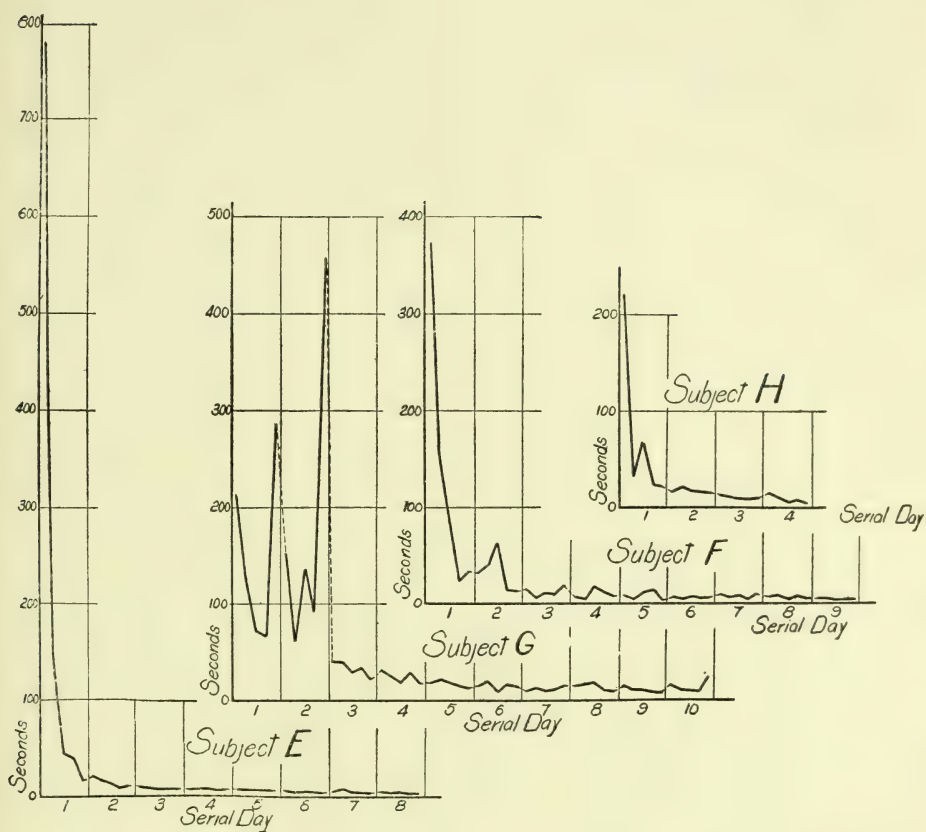
Figs. 18-21. Learning with Maze M, Series I. Subjects A, B, C, and D. Solid line is for times with maze obscured from view. Dotted lines, for breaks between sessions. Small numbers in circles, for number of intervening trials with maze visible. Dots below, for actual times of visual trials.



Figs. 22-25. Learning with Maze M, Series I. Subjects E, F, G, and H. Solid line is for times with maze obscured from view. Dotted lines, for breaks between sessions. Small numbers in circles, for number of intervening trials with maze visible. Dots below, for actual times of visual trials.



Figs. 26-29. Learning with Maze N, Series II. Subjects A, B, C, and D. Maze obscured from view throughout. Dotted lines indicate breaks between sessions.



Figs. 30-33. Learning with Maze N, Series II. Subjects E, F, G, and H. Maze obscured from view throughout. Dotted lines indicate breaks between sessions.

TABLE XVII

MAZE N—MAZE OBSCURED FROM VIEW

Figures show average times in seconds for five trials on serial days 48 hours apart. Rank of subject is based on days 2 to 8 inclusive (except for H).

Serial Day	Subject							
	A	B	C	D	E	F	G	H
1	341.8	56.4	182.0	146.4	204.4	136.2	153.2	72.6
2	88.8	297.2	37.6	102.8	14.6	33.6	179.8	17.2
3	36.8	65.2	32.4	38.4	8.0	13.6	34.0	10.0
4	56.6	32.8	32.0	70.8	7.0	11.2	26.0	9.0
5	23.6	14.4	21.2	31.0	6.8	9.2	17.8	
6	20.4	14.2	18.4	21.0	5.0	6.8	16.2	
7	16.6	13.8	14.6	18.4	6.0	7.8	12.6	
8	12.2	11.2	12.4	14.8	5.0	7.6	15.0	
9	10.4	12.8	7.8	14.6		6.2	11.0	
10	10.2	11.8	7.4	11.4			13.6	
11	8.6	13.8		12.8				
12	7.8	10.0		13.2				
Rank of Subject	7	6	4	8	1	3	5	2

#### IV. RUG-MAKING

##### PROCEDURE AND METHOD OF QUANTIFICATION

We shall now proceed to examine the performances of these subjects in the learning of a practical mechanical operation such as is involved in the simpler forms of skilled industrial work. For this purpose the making of hooked rugs was selected as being not difficult to learn and as providing an operation, the product of which was readily quantifiable. It was hoped, in fact, that there might prove to be no difference in the quality of the rugs, so that the only variable to be considered would be the speed of making. This hope, however, was not fully substantiated, although the differences in quality, with one exception, were not great.

The hooked rugs are made in the following manner. A piece of ordinary burlap or sacking is stretched and tacked upon a rectangular wooden frame, and forms thus the base for the rug. The rug is worked into this with woolen strips cut from old army uniforms. The only tool required is a hook, made of 3/16-inch steel rod, bent at one end into a loop that serves for a handle. The other end is filed down to a taper, about 3/32 of an inch at the end, and is then filed well in at one side, so that a small knob remains projecting at the end on one side of the tapered point. The whole hook is

about five inches long. The rags to be used in the rugs are cut or torn into strips about half an inch wide. A strip is held in the left hand underneath the frame, the hook is thrust through the burlap, the strip is laid smoothly over it upon the hooked side, and the hook is pulled through a loop of the cloth. The loop is pulled well through, straightened out, if crooked, and pulled back by the left hand down over the hook stem, which is turned with the hook side away from the strip. The loop is, of course, not pulled back through the burlap, but against the hook above the burlap, thus being adjusted smoothly at a uniform height. The hook is removed from the loop thus made and thrust through the burlap a few threads further on, where another loop is pulled through, close against the first. Thus the process continues. When a new strip of cloth is begun or the one in use finished, the end is pulled through the burlap and cut off the same height as the loops.

The experimental rugs were made 32 by 18 inches. It was planned to make them perfectly plain without any pattern at all, in order that they might be equally difficult throughout. After commencing work, however, it was discovered that the work was so monotonous that the subjects before long grew tired of

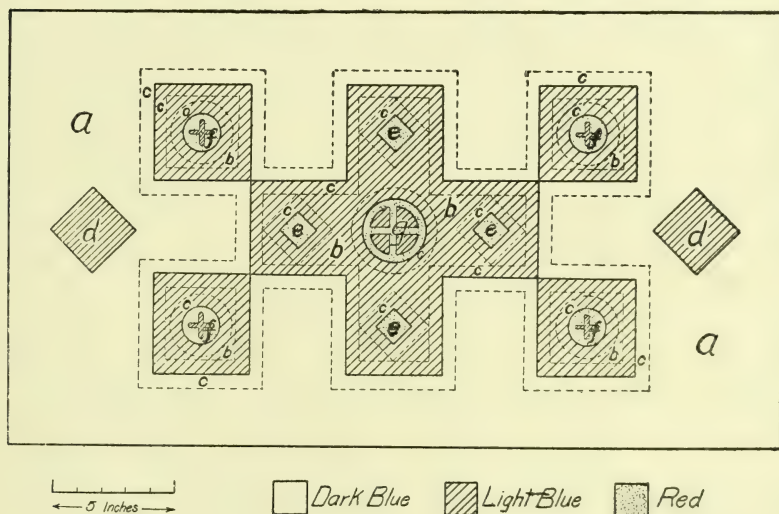


Fig. 34. Rug Pattern. Letters indicate relative difficulty of parts, "a" being the easiest, "g", the hardest. See text and Table XVIII.

it, and it soon became a question whether they could be kept at it long enough to finish the rugs. Accordingly a pattern was

introduced in order to stimulate interest, and later the pattern was complicated somewhat for the same purpose. The pattern, which was not designed for artistic effect, included large squares, a figure intended not materially to complicate the problem. The details of the center were added still later. The design is shown in Fig. 34. Blue army coats were used for the base, light blue army capes and trousers, and in one a gray cape, for the center, and red flannel for the small figures.

In general all subjects followed the same order in making the different parts of the rug. First the plain outside field (a, Fig. 34) was filled in as far as the diamonds (d). Then these diamonds were worked and the outside filled in all around the rug to within one or two inches of the large central form. Then in order that there might be a clear-cut line between the outer and the central parts, the central portion was outlined in dark blue (c). The outside was then filled in to the outline mark. Next the central portion was again outlined, this time on the inside, by light blue (c), thus assuring the clear-cut line of demarcation between the two colors. Next the diamonds and circles were outlined in light blue (c), and then the rest of the center was filled (b). After this the small diamonds were filled in in red (e), then the small circles, in red with a blue cross in center (f), and finally the large central circle with a red cross and blue filling (g) was worked.

It may be objected that the pattern finally adopted was by no means even nearly equally difficult throughout. This is true, but it is not thought that the matter is nearly as serious as at first appears. After they had completed the rugs, the subjects were questioned as to their opinion of the relative difficulty of the different parts of the rug. Four of the five who completed rugs agreed exactly in ranking the different parts in difficulty; the fifth did not differ by more than one grade from the others for any one part. The letters designating the parts in Fig. 34 are arranged so that they indicate the difficulty of the part according to the estimate of the subjects, "a" being the easiest, "g", the hardest. The reasons for the differences are readily

understood. The filling in of the outside part (a) is plain, regular work and is naturally comparatively easy. The filling in of the center (b) is slightly more difficult, because by the time the center is reached the rug has been largely filled in and the burlap is consequently stretched tighter, so that it is more difficult to pull the loops through. There are also more small corners to be worked in the center. The outlining of parts (c) is again more difficult because care must be taken to keep one edge of the row worked even. The large diamonds (d) are difficult because they have to be kept straight and uniform; the small diamonds (e) are more so, because in addition the burlap is tighter in the center and because the red flannel proved harder to loop. The small circles (f) are still less easy, because they contain a small cross; and the large circle (g) is most difficult of all because of the additional detail that it involves. If, however, we examine Table XVIII, in which the approximate areas of each part of the rug are given we find that the difficult portions make up only a very small proportion of the entire rug. Parts "a", "b", and "c" are all regular work and differ only slightly from one another in difficulty. They make up, however, 94.6% of the entire rug. Part "a" alone forms 60.5% of the rug. The difficult details, "d", "e", "f", and "g", constitute only 5.4%. Obviously then, if we wish to study learning, we have only to neglect the last six hundredths of the operation, in order to have the whole process nearly equally difficult.

TABLE XVIII

## RUG AREAS

Approximate areas of the different parts of the rug arranged in order of difficulty. Letters are those used in text and in figure 34.

Letter	Part of Rug	Area	
		Sq. in.	%
a	Filling in outside.....	351	60.5
b	Filling in center .....	24	4.2
c	Outlining center.....	172	29.9
d	Large diamonds .....	13	2.6
e	Small diamonds .....	4	.7
f	Small circles .....	7	1.2
g	Large circle .....	5	.9

} 94.6%

} 5.4%

The series were begun with the subjects working alone with the experimenter for fifty minutes every other day,—all the time

available by the experimenter. It soon appeared, however, that, in order to finish the rugs within a reasonable time as well as to keep up the interest of the subjects, it would be necessary to work much longer and oftener. Accordingly it was decided to allow all the subjects to work together in a large room. The periods were increased to 150 minutes and the subjects worked every afternoon except Sunday. This change was made on the fifth day for all subjects except A and H, for whom it was made on the second day.

It is thought that for most of the subjects the working together acted very little as a distraction. The refusal of subject G to work shortly after the change may have been partly due to the fact that he was ashamed to be seen employed in industrial work, but there must have been other reasons, as he also refused to continue the cancellation test. The other patients, except H, came after a while to converse more or less freely and to seem not averse to the social feature that was thus introduced into the work.

During the first part of the work the experimenter worked regularly at a rug, while the patients worked. This was done for three reasons. In the first place, it was necessary for the experimenter to understand the operation thoroughly, in order to assist and instruct the subjects. In the second place, it seemed advisable, in order to secure the continued co-operation of the subjects, who, it must be remembered, are generally averse to performing manual work of this character, and who, in this case, had little incentive to work, to take rug-making off the plane of manual labor for the benefit of the hospital, and to mark it as an operation which, for "scientific purposes", it was desirable for even the experimenter to do. In the third place, it was thus made possible to use the record of the rug made by the experimenter as a control. The experimenter, as a rug-maker, is referred to as "X". Working always whenever the subjects worked, his series is somewhat different from the others. At first he works 100 and 150 minutes on alternate days, his results, however, being computed for each fifty-minute period. On the ninth day and thereafter he worked the 150-minute periods every afternoon.

The record for each subject shows the number of loops made each day and the part of the rug worked, the latter being recorded on an outline map of the rug. In counting loops each free end at the beginning or end of a strip was counted as a full loop, since approximately as much effort was required to pull the end through as to pull a loop. The subjects cut and tore their own strips. No account was taken of the time consumed in this operation, as it was small. It was assumed that it was about equal for all subjects.

### RESULTS

The results are shown in Table XIX. Here are given the number of loops per minute made in each working period the value being placed opposite the total amount of time spent on the rug up to the end of the period considered. The length of the period, for which each figure given is the average rate, is the difference between the total time given for that period and the total time for the last entry made. Work done in 50-minute periods is, however, marked by an "x", that done in 100-minute periods by "xx". All other work was in 150-minute periods, except in some cases that done on the final day. The table also shows the part of the rug worked upon in the particular period, the part being referred to by letter (Fig. 34 and Table XVIII). Its rather broken form is due to the attempt to combine three different serial orders for purposes of comparison.

The data of Table XIX are much more readily comprehended by reference to the curves of Figs. 35-42. Here the letters at each point indicate the part of the rug worked upon. As before, "x" signifies that the work was done in a 50-minute period, "xx" in a 100-minute period, while uncrossed dots indicate 150-minute periods. The curves must be interpreted only in connection with the letters. In general, the rate tends to fall off at the end of the series, but this is because the more difficult parts of the rug are recorded there. If a drop in the curve is accompanied by the introduction of a letter indicating that a more difficult part was begun, it does not indicate that there was not actual improvement. Take, for example, the curve of subject

TABLE XIX

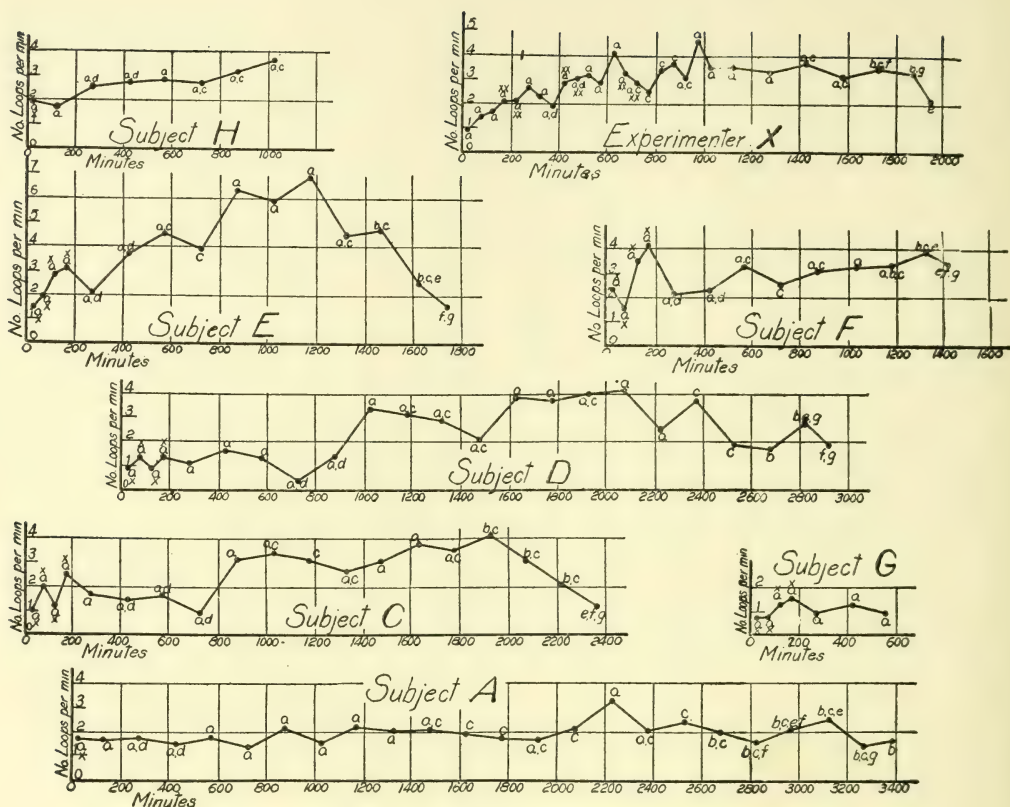
RUG-MAKING

Figures show rate of working, in number loops per min., in relation to total time of working in min. Letters refer to parts of rug worked upon; see Fig. 34 and Table XVIII. x = 50-minute working period; xx = 100-minute period. All other periods are 150 minutes, except on final day for each subject.

## Subjects

Total no. min.	A		C		D		E		F		G		H		X	
	Rate	Pt.	Rate	Pt.	Rate	Pt.	Rate	Pt.	Rate	Pt.	Rate	Pt.	Rate	Pt.	Rate	Pt.
50	1.72x	a	.96x	a	.84x	a	1.52x	a	2.26x	a	.66x	a	1.96x	a	.98	a
100			1.86x	a	1.34x	a	1.98x	a	1.58x	a	.74x	a			1.42	a
150			1.20x	a	.84x	a	2.84x	a	3.52x	a	1.34x	a			1.64	a
200	1.69	a	2.42x	a	1.30x	a	3.08x	a	4.04x	a	1.54x	a	1.75	a	2.04xx	a
250															2.10xx	a
300															2.62	a
350	1.70	ad	1.71	a	1.07	a	2.09	ad	2.18	ad	.97	a	2.58	ad	2.34	a
400															1.92	ad
450															2.84xx	a
500	1.45	ad	1.43	ad	1.61	a	3.75	ad	2.27	ad	1.29	a	2.89	ad	3.08xx	ad
550															3.20	a
600											.86	a			2.92	a
650	1.75	a	1.52	ad	1.35	a	4.44	ac	3.29	ac			2.92	a	4.20	a
700															3.34xx	a
750															2.84xx	ac
800	1.39	a	.94	ad	.37	ad	3.92	c	2.59	c			2.77	ac	2.48	c
850															3.40	c
900															3.68	c
950	2.06	a	3.10	a	1.39	ad	6.30	a	3.13	ac			3.22	ac	3.12	ac





Figs. 35-42. Learning in Rug-making. Seven subjects, and X as control. Letters indicate relative difficulty of parts, "a" the easiest, "g", the hardest. See Table XVIII, Fig. 34, and text.  $\times$  = work done in 50-minute period.  $\times\times$  = work done in 100-minute period. All other work, except on final day, done in 150-minute periods.

E, which shows learning, although it ends about on a level with the start. Throughout the first four days there is steady improvement. On the fifth day "d" is introduced and the rate falls off, but recovers the next day in spite of "d". The following session some "c" does not prevent improvement, but on the next day the subject works at "c" all the time and goes more slowly. Practice in "d" is different and is thus probably not beneficial for working "c". When "c" is dropped and only "a" is worked, again there

is a sudden rise, this time to the peak of the curve, the dent in the top of which is due to some unexplained cause. When "c" is reintroduced, however, there is again a drop, which goes still farther with the introduction of "e", "f", and "g". When interpreted in this manner the curves of subjects E, F, H, and X, all show learning. C does also, except for the next to the last two days. G did not continue long enough to develop marked improvement. D, the most erratic of the subjects in all the tests, shows sudden improvement after 750 minutes, which continues almost to the end. This improvement appears to be independent of the relative difficulty of parts. It is probably due to the fact that the low points on the curve are caused not so much by any intrinsic difficulty in the problem, as by lapses of attention in the subject. Subject A alone shows no increase in rate, but it cannot be said that he does not improve, since he is able to maintain the same rate in the more difficult parts as in the easier portions of the rug.

It has been remarked that the rugs did not all prove to be alike in quality of workmanship. The rug of F, for example, was much poorer than the rest (unless the end of a rug made by G be expected), because he insisted on working rapidly and carelessly making very large loops. Although he finished in about 80% of the time of his nearest competitor, he put in only about 70% as many loops. Upon casual inspection the other rugs were, however, not markedly different. Accordingly it was decided to grade them in quality by having them judged by a number of normal people. Ten men and five women, most of them attendants and nurses in the hospital, were asked to arrange the rugs in a series according to the quality of workmanship, neatness, regularity, and apparent carefulness of construction. The partly worked rugs of G and H were also placed in the series as exactly as possible, by comparison with the corresponding parts of the other rugs. The vote resulted in a remarkable uniformity of opinion. The average ranks of the eight rugs with the mean variations are given in Table XX. The mean variations are small; in only two cases are they greater than a single unit of

rank. Four of the rugs were placed in exactly the same position by more than half of the observers.

TABLE XX  
QUALITY OF RUGS

Figures are average ranks and mean variations, based upon the opinions of 15 normal persons, each of whom ranked the rugs independently.

Subject	A	C	D	E	F	G	H	X
Av. rank	5.3	2.9	4.6	1.4	7.1	7.7	4.4	2.6
M. V.	±.89	±1.08	±1.06	±.56	±.34	±.46	±.68	±.66

It had seemed possible that the number of loops made per square inch might be a measure of the quality, not that mere closeness of stitches would make for excellence, but that it might be an index of general carefulness. This relation, however, does not seem to hold although there may be some slight interdependence. The correlation between quality, as determined by the vote, and closeness of loops is 30.9%. (Pearson: method of rank-differences.) It is interesting also to note that the closeness of the loops appears to be almost entirely independent of the speed of working, the correlation being—14.1%,—the negative sign indicating perhaps a slight tendency to place the loops farther apart when working fast.

In Table XXI we compare the ranks of the subjects in rapidity and in quality of work. It will be observed that the two vary similarly. It seems that the workers who work rapidly, work also well. The correlation between the two factors is 73.8%. F has already been noted as a marked exception to this rule, a fact which has been borne out by other rugs that he has since made. Some of these later rugs were made so rapidly as to be hardly fit for use; while others in which he was constantly admonished to go slowly were quite creditable. If F be excluded in computing the correlation between speed and accuracy, the value is 90.6%.

It is interesting to note that the rug of the experimenter (X) ranks, not first, but second both in quality and speed. One subject surpassed and several nearly equaled the experimenter, indicating that the operation could be learned by some of the dementia precox patients almost as readily as by a normal subject. The experimenter, it may be added, while perhaps not naturally adept

in operations requiring manual dexterity, has had some manual training in other lines of work.

TABLE XXI

COMPARATIVE RANKS OF SUBJECTS IN QUALITY AND SPEED IN RUG-MAKING Basis of ranking	Subjects							
	A	C	D	E	F	G	H	X
Quality .....	6	3	5	1	7	8	4	2
Speed .....	7	5	6	1	3	8	4	2

Correlation = 73.8%

### CHARACTER OF THE OPERATION

In order that the exact character, on the conscious side, of the operation of rug-making might be thoroughly understood and compared with the processes of learning already described, introspections on the operation were obtained from the subjects after they had completed about twenty hours of work, that is to say, at the time when the regular filling in had been learned as well as possible and before the more difficult parts were begun. These introspective reports, it must be remembered, were made at the time that the subjects were beginning to give their most satisfactory reports on the maze and the subjects apparently comprehended the meaning of the questions in a way that would never have been possible without considerable preliminary training. With regard to the rug-making, the subjects were first asked to describe their method of procedure and were then questioned in order to determine as far as possible what parts of the operation were carried consciously and the sorts of perceptual cues and imagery that were present.

The manner of questioning the subjects and the kind of answers elicited have already been fully shown under the introspections for the maze. We shall content ourselves here with giving rather fully an account based upon the introspections of the experimenter, made during the rug-making period. This will be supplemented by a brief statement of the report of each subject.

*Experimenter (X).* The position of each loop in the burlap is determined by a visual perceptual cue. The hook is thrust through and the hole thus made enlarged either automatically or with unclear kinesthesia. The arrange-

ment of the cloth strip on the hook beneath the burlap is accompanied by unclear kinesthetic sensations and a clear visual image of the strip, the latter carrying the meaning that the strip is being brought toward or away from the experimenter, as the case may be. [Since the direction of working the strip was frequently altered and the arrangement upon the hook is dependent upon the direction, it is not surprising that the processes having this meaning should remain clear.] The bringing of the hook through the burlap is sometimes automatic, sometimes accompanied by unclear muscular sensations in the hand and arm. These sensations occasionally become clear when difficulty is encountered in getting the loop through. There is also a more or less clear visual perception of the loop and of the end of the hook, which persists until the loop is adjusted. The pulling down of the loop over the hook is almost invariably automatic.

*Subject A.* S makes a definite judgment, apparently in visual terms, of the position of the loop, and then sticks the hook through, arranges the cloth upon it, and pulls it up automatically,—at least S insists that he never thinks about these movements. The arrangement of the loop after it is pulled through involves visual, kinesthetic, and probably other processes.

*Subject C.* The determination of the position of the loop appears to be largely visual, the arrangement of the strip beneath the burlap and the pulling of the loop through, automatic. The loop is adjusted with at least an unclear visual accompaniment, which becomes clear whenever there is difficulty in arranging it properly.

*Subject D.* The position of each loop is located by visual perceptual cues. The hole is made and enlarged and the strip arranged on the hook with clear kinesthetic sensations determining the adjustment. S is uncertain about the rest of the operation.

*Subject E.* A definite visual perception precedes the determination of the place where the hook shall be thrust through. The thrust is automatic, but the placing of the strip upon the hook is always accompanied by kinesthetic sensations in the fingers and nearly always by a visual image of the cloth. Unclear kinesthesia and probably an unclear visual perception accompanies the pulling through of the loop, the visual perception becoming clear whenever difficulty that calls for a slight change of adjustment is met. The adjustment of the loop is carried in clear kinesthetic and visual processes. The pulling of it taut by the left hand, however, is entirely automatic.

*Subject F.* The position of each loop is determined by a visual perception. It is impossible to say what the later processes may be, as S, showing a different attitude than that taken toward the maze and cancellation forms, takes great pride in the care with which he works, so that he will not admit that any operations at all are done without the most careful attention. He cannot, however, describe 'what his thinking is like' in these cases.

*Subject H.* The report of this subject is also not clear. The position of the loop is determined in visual terms. Probably a large part of the rest of the operation is conscious, although the type of processes cannot be told with certainty.

The subjects are apparently all very much alike. The simpler

movements are early guided by kinesthesia and later become automatic. The more complicated movements or those movements that have to be varied from time to time according to chance conditions—the various adjustments of the cloth—are accompanied by a mixed perception of which the sensory part may be kinesthetic or, if the operation is in sight, visual, and the imaginal part (if the interpretation of the reports is correct) almost entirely visual. When conditions interfere with the performance of an automatic movement, the altered movement is clearly conscious. The rug-making experience on the conscious side thus combines, in a rather complex way, elements involved in the learning of both the cancellation forms and the mazes.

## COMPARISON OF RESULTS

From time to time we have ranked subjects in the various tests performed. As a matter of final interest we have brought together in Table XXII the ranks of the subjects in all the tests.

TABLE XXII

RANKS OF SUBJECTS IN DIFFERENT TESTS

Tests	Subjects							
	A	B	C	D	E	F	G	H
Attention .....	1	4.5	2.5	8	6	4.5	2.5	7
Immediate memory .....	5	5	5	8	5	1.5	5	1.5
Apperception .....	2	7	5	8	6	4	3	1
Directions .....	1	3.5	7	8	5	3.5	6	2
Tapping (speed) .....	4	7	4	8	1	6	4	2
Aiming (accuracy) .....	6	2	7	3	1	8	4	5
Kinesthetic memory .....	7.5	5	7.5	3.5	1	2	6	3.5
Cancellation (speed) .....	3	8	7	6	1	5	4	2
Cancellation (accuracy) .....	4	8	1	6	5	7	3	2
Maze (speed) .....	7	6	4	8	1	3	5	2
Rug-making (speed) .....	6		4	5	1	2	7	3
Rug-making (accuracy) .....	5		2	4	1	6	7	3

The table does not show quite as much variation between the tests as might be expected. E, for example, does well in most of the tests, while D is almost always very poor. This fact is shown numerically by the average correlation of the sixty-six possible combinations in pairs of the twelve tests. This correlation is

+21.9%. It does not, however, necessarily mean that one observer has a tendency to excel in all the factors of all tests, another to do poorly. The fact that the majority of tests are of a motor character suggests that the uniformity may be the result of excellence in a common factor.

It proves, however, not to be possible to prophesy the ability of a subject in one test on the basis of his ability in another test. We find many apparent inconsistencies. For example, accuracy in cancellation and accuracy in rug-making are both highly correlated with the speed of making dots in the tapping test, but they are not highly correlated with one another. Such high correlations, if significant at all, cannot indicate excellence merely in some single factor, but must be the result of a complex interplay of at least several factors. In such a case, correlation of two factors with a third might occur without there being a correlation between the two factors themselves. This view is supported if we consider the speed tests alone. Speed of tapping, of cancellation, and of running the maze are all highly correlated with each other. Speed of making rugs shows a high correlation with the maze speed, but does not show high correlation with the speed of tapping or of cancellation. Possibly it is significant that the apparent inconsistency occurs with the more complicated operation, that of rug-making.

The correlations greater than fifty per cent. for the last five items of Table XXII are as follows:

Time of cancellation:—tapping, 82% ; directions, 65% ; immediate memory, 58% ; apperception, 55% ; maze, 55%.

Accuracy of cancellation:—tapping, 62% ; apperception, 57%.

Time of maze:—rug speed, 82% ; tapping, 76% ; immediate memory, 71% ; kinesthetic memory, 55% ; time of cancellation, 55% ; rug accuracy, 51%.

Rug speed:—time of maze, 82% ; kinesthetic memory, 79% ; rug accuracy, 59% ; immediate memory, 51%.

Rug accuracy:—rug speed, 59% ; tapping, 57% ; time of maze, 50%.

It will be seen that the cancellation test, the maze test, the tapping test, and the rug-making are all rather closely related.

if the similar ranking of the subjects can be taken to indicate this fact, but that it is not possible to determine exactly what that relation is or to factor the processes ino terms of each other with the data that we have at hand.

## EFFECT OF OCCUPATION UPON THE SUBJECT

Therapeutic value has often been claimed for the employment of the insane. It is not possible, however, in the present case to determine the exact effect of the work upon the patients. In the first place, the time was too short and the number of patients too few to justify a generalization. In the second place, the extra-experimental conditions were not controlled. A change in the general condition of the patient during the period of experimentation might have been caused by some change in the hospital routine affecting him outside of the experimental hours. In the third place, it is not possible to separate the effect of employment from the effect of sympathetic association with the experimenter, who was able to form a much closer, personal relation with the patients than was possible for the nurses and attendants.

In some of the subjects there were definite alterations of disposition as the work progressed. In spite of the difficulty in interpreting these changes, it is thought worth while to record them.

Subject A was at first dull and apathetic in the experimental work and obstinate in the ward, having to be tube-fed. Later he became much interested in the rug-making and would sometimes talk about it, while in the ward he was most tractable. He had, however, been subject to such changes in disposition before, and it is possible that the period of good behavior was helped on by his removal during the period of experimentation to a ward that he liked especially well.

Subject B showed little change. He was not employed in rug-making.

Subject C became more cheerful and less bothered by worries as the experimental period progressed. His manner also became less apathetic. He was, however, allowed a limited parole at

about the time the change appeared, and this may have helped his improvement.

Subject D became much interested in the rug-making and frequently expressed his pleasure in it. He was no longer mute or apathetic, as he had been early in the experimental period. His manner while in the ward remained unchanged, however.

Subject E at first complained very much of being imprisoned, asked constantly for parole, and talked all the time about his troubles and his hallucinations. He became later very much interested in all the work, no longer complained, and stated that his hallucinations had become very infrequent.

Subject F was always cheerful. He conceived a great fondness for rug-making, and made many more rugs after he had completed the experimental one. There was, however, little change in his condition, except that his characteristic inane laugh became very infrequent.

Subject G remained about the same throughout the period, except for two excited spells. The first was manifested only in the ward, and did not affect his experimental work. The second was connected entirely with the experimental work, and resulted in his refusal to continue with the rug-making and the cancellation test.

Subject H remained consistently complaining throughout, and worried constantly over trivial matters. Among other things he complained of the experimental work, and finally refused to do it altogether.

It appears, then, that Subjects B, F, G, and H showed no improvement in condition during the experimental period; that A and C showed decided improvement, which may, however, have been contributed to by outside causes, and that D and E showed decided improvement without there having been any obvious change in outside conditions, although the improvement in D was manifest only during the hours of experimentation. The most striking case was that of E, who, it will be recalled, excelled in almost all of the tests except those given at the very beginning of the experimental work.

## CONCLUSIONS

1. Dementia precox patients, can be readily trained in the performance of simple tests of learning or of more complex operations of an industrial nature.

2. These patients are available as subjects for psychological investigation, provided the apparatus used is not too complicated. They can be trained without great difficulty to give introspective reports of the more prominent features of conscious experience.

3. The patients show large individual differences in ability to learn the operations, in manner of procedure, and in the consciousness accompanying the performance.

4. The patients are capable of fairly precise motor adjustments. The accuracy of a very simple motor adjustment does not increase with practice, but does depend upon the extent and the speed of movement. In the latter case accuracy decreases more rapidly than speed increases.

5. There is but little evidence for transfer of practice from an operation that is but slightly motor in character.

6. The course of consciousness in learning a maze is that of the normal subject. Verbal, visual, and attitudinal factors are usually replaced by kinesthetic, which turn lapse as the movement becomes automatic.

7. There is indication that employment may be beneficial to some patients, although this cannot be asserted positively.

8. The patients are capable, in simple industrial operations, of a quality of work, sufficiently good to be commercially valuable.



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## An Experiment in Linear Space Perception

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## EXPERIMENTAL PROCEDURE

The purpose of the experiment under consideration has been to apply the statistical methods outlined in F. M. Urban's monograph on Statistical Methods in Psychophysics<sup>1</sup> in another field, viz., the discrimination of small differences in line lengths. Urban's methods have thrown new light on psychophysical problems and especially upon the interrelation of the different methods of psychophysical experimentation and the interpretation of results. His work is based upon experiments with lifted weights. As the present experiment was intentionally arranged to lend itself to similar treatment, much of the detail of laboratory procedure was made to conform, as nearly as possible in a different field, to the plan of the earlier experiments.

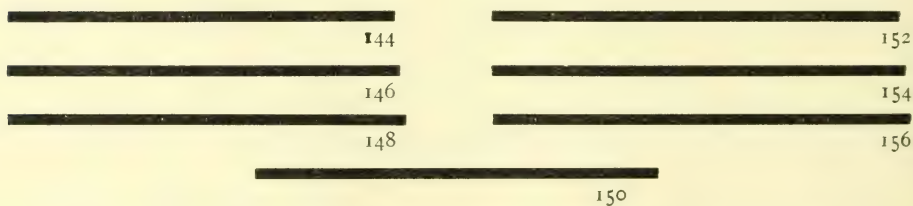
Urban used seven comparison weights of 84, 88, 92, 96, 100, 104, and 108 grams respectively. These were placed along the edge of a circular turning-table 3 feet in diameter alternately with seven standard weights of 100 grams each. The subject sat at the table so that his hand was just above each weight in turn, as the table was revolved by the experimenter. He lifted a standard weight and then a comparison weight, making the judgment "heavier", "lighter", or "equal", which was recorded by a third person. The details of this procedure are given fully in the monograph referred to above and where there has been an important departure from them in the present procedure, the reasons for the variation are indicated below.

The judgments given in this experiment were based on a visual comparison of black lines printed on white cards and presented to the subject in pairs. Seven comparison lines were used. Each pair, a comparison line and a standard line, was printed on a separate card  $3\frac{1}{2}$  by 12 inches, lengthwise of the card, in its median line, and one inch apart. Care was taken to center

<sup>1</sup> The Application of Statistical Methods to the Problems of Psychophysics; F. M. Urban, Ph.D. (Psychological Clinic Press, Philadelphia, Penna. 1908.)

these so that the adjacent ends of the lines were each  $\frac{1}{2}$  inch from the center of the card. The variation of the distance between the end of the line and the end of the card was less important, amounting to only about 2 per cent. Moreover the space between the lines was in the field of attention, whereas the other spaces were not.

A standard stimulus of a black 4-point line, 150 printers' "points" long, was used. There were seven comparison stimuli, also 4-point lines, of 144, 146, 148, 150, 152, 154, and 156 points respectively. These lines were printed on white cardboard, several sets being prepared so that it might not be necessary to use soiled or marked cards. These lengths were chosen after extensive preliminary experiments with more extended series having greater differences between the comparison stimuli. The printers' point ( $\frac{1}{72}$  inch) was used as a unit of measurement because the lines could then be cut more accurately by the cutting-machine of a printers' supply-house than by the hand methods necessary if any other scale had been used. The difference between the two comparison lines at the extremes of the series is therefore  $\frac{1}{6}$  of an inch, or 8 per cent of the standard in comparison with Urban's range of 24 per cent in lifted weights. A 4-point line was chosen on the ground that a wider line would introduce an area judgment, the line appearing as a black rectangle, and that slight variations of illumination or of optical defect would become disturbing factors with a narrower line. Specimens of the lines used are given here.



These cards were presented to the subject by a machine designed particularly for this purpose. A drum of light frame in the form of a fourteen-sided prism was rotated by an inter-

mittent gear connected with a speed-reducer and motor. A card was fastened in place upon every other face of this prism, the intervening spaces being covered with white paper to match the cards. Each card in succession was brought to rest for about  $1\frac{1}{2}$  seconds behind an opening  $2\frac{1}{2}$  by 10 inches in a cardboard screen which cut off all view of the apparatus from the subject who sat at a small table seven feet from the screen. The opening was on a level with the subject's eyes. The screen and card were lighted by a large window behind the subject. There were no other windows in the room, the walls were of a neutral tone, and there was little to distract the attention of the subject. There was a slight whirring noise from the motor and a click as the drive-wheel picked up the gear-wheel on each revolution; otherwise the room was quiet.

The cards were held firmly in place on the drum but could be withdrawn easily when it was necessary to replace them or to change the order. The seven card positions were marked plainly with the numbers 1 to 7 on the end of the drum. Five series of cards were prepared, each being made up of seven similar cards arranged in a different order. Each card in a given series was inconspicuously marked at one end with its series number and with the number of the face of the drum on which it was placed (e. g., Series III, Number 6), but bore no mark by which a subject could identify it when examining the apparatus. These marks were at the extreme margin and so could not be seen by the subject during the experiment. As the marked end of the card was always placed toward the numbered end of the drum, it was easy to avoid placing a card in a wrong position.

As the subject faced the apparatus he was directed to lean forward, sit up straight, or lean back in his chair according as one or the other position seemed to give him the best view of the lines. If he ordinarily wore glasses, he retained them. He was told to direct his attention to the *left-hand* line, the standard, and then to carry his eyes across to the right and give his attention to the other line, the comparison stimulus. He then pronounced aloud his judgment as "longer", "shorter", or "equal",

the record being made by a second person. All subjects used the word "equal" but some preferred the word "greater" for "longer" judgments and "less" for "shorter" judgments. Each was encouraged to use the word which most promptly and spontaneously fitted his judgment. In a few cases a subject would revise his judgment, saying for example: "Longer; no, I mean equal." In such cases the second or final judgment was recorded. In the early conduct of the experiment a few of the subjects showed a tendency to omit judgments on single pairs of lines here and there in a series. It was necessary to caution recorders to observe closely that all seven were judged. Where one of a series of seven was omitted, the remaining six judgments on the pairs 1 to 7 were thrown out. The recorder sat where he could control the motor-switch and watch the apparatus; and particularly so that he could see the numbers on the drum. The subject began to give judgments as soon after he was seated as it was possible for him to get a good view of the opening in the screen or, if the apparatus was not in motion, as soon as the movement began. Wherever in a series a subject began to give his judgments, the recorder always made his first record on the pair numbered 1. In this way it was possible for subject and recorder to interchange places since neither knew the order of the comparison stimuli and since the observer could not even tell when he began to give judgments on the first comparison pairs. On the record sheets the pairs were known only by their series and order numbers so that two men acting alternately as subject and recorder could have completed all five series without knowing either the absolute or the relative lengths of the comparison lines. However the experimenter himself recorded some of the judgments of most of the subjects and in some cases all of them. He took special pains to do this in cases where there appeared to be something unusual in the general trend of the subject's judgments. The records were made on sheets printed for this purpose. Each record-sheet had a space at the top for the number of the series, the name of the subject, and the date of the experiment. Seven lines, numbered from 1 to 7, were carried across the page and

were divided into squares so that the recorder filled in the squares of one column and then of the next as the cards numbered 1 to 7 came into place and were judged by the subject. The letters *g*, *e*, and *s* were used for longer, equal, and shorter, respectively, *g* being used in place of *l* since preliminary experiments showed that *l* was easily confused with *e* in the handwriting of some recorders. When ten, or in some cases fifteen, of these columns had been filled, the subject was allowed to rest or exchanged places with the recorder. The following is a transcript from an actual record-sheet.

*Name*—Mr. Smith*Series*—III*Date*—Mar. 25, 1911

1	s	g	g	e	g	g	g	g	g	g	s	e	g	e			
2	g	g	s	e	e	g	s	s	e	g	e	s	s	s	g		
3	g	g	g	g	g	g	g	g	g	s	g	g	g	g	g		
4	g	g	g	s	s	g	s	g	g	g	g	g	g	s	g		
5	g	g	g	g	g	g	g	g	g	g	g	g	g	g	e		
6	g	g	g	g	g	e	g	s	g	s	e	g	g	g	g		
7	s	s	s	s	s	s	s	s	s	s	s	s	s	g	s		

In the preliminary experiments various devices were tried by which the subject might record his own judgments, but none of them proved in the least degree satisfactory. On the whole the method employed commends itself for use in future experimentation.

Eleven of the twelve subjects were men. The other was a woman, a teacher in a private-school near Philadelphia. Of the men, one was Dr. Urban himself, to whose sympathetic co-operation the writer is indebted, two were graduate students, and eight undergraduate students at the University of Pennsylvania. Their ages probably ranged from eighteen to thirty-five years. Six of the undergraduate students were employed as subjects and were paid for their services. The other six were in a sense voluntary subjects although three of them took part in this experiment as a part of their psychological laboratory work in course.

Where subjects had no previous knowledge of the nature of the experiment, little attempt was made to explain its different phases. Enough information was given to elicit as high a degree of attention as possible and very careful and particular directions were given in regard to the manner of looking at the cards and of giving judgments. Though the experiment was not concerned with the *rightness* or *wrongness* of the judgments, subjects were sometimes told that they were "doing well" or that they were "improving" when this was the case, or that "one man was doing better than another". The purpose of this was to secure the highest degree of attention. The subjects were not informed that the seven cards of one series were the same as the seven cards of another but in different orders, though many of them knew it. Nor on the other hand was such information withheld where subjects became interested in the experiment. In some cases subjects were shown the results of their first series before later series were completed. Although some of the subjects were employed, the experimenter has every reason to believe that, as far as attention could be controlled, each subject did his best to concentrate upon the experiment and to follow out the instructions in regard to making judgments. For reasons discussed later, no effort was made to train subjects for this experiment though three of the subjects had had a considerable amount of training in psychological laboratory work. As will appear later in the discussion of results, there are some drawbacks to having subjects with no training on the particular experiment undertaken even if they may have had training in other psychological laboratory work. Training was omitted in this series of experiments partly on account of the difficulty of keeping the training element the same in all subjects and partly because in these experiments it seemed desirable to see to what extent they could be used as a sensitivity test of a new subject with no training.

The seven cards, as previously stated, were arranged in five series. The orders of the comparison lines in these series were as follows:

	<i>Series I</i>	<i>Series II</i>	<i>Series III</i>	<i>Series IV</i>	<i>Series V</i>
1. ....	146	156	152	150	144
2. ....	154	148	146	156	152
3. ....	150	146	154	152	156
4. ....	156	152	148	146	146
5. ....	144	144	156	144	150
6. ....	148	154	150	148	154
7. ....	152	150	144	154	148

Since a subject did not know when he was judging Number 1, Series I might just as well read 150, 156, 144, 148, 152, 146, 154, 150, 156, 144, etc. All the subjects testified that they had no sense of a series of seven beginning at a particular card and again beginning there. This was true even with those subjects familiar with the general plan of these series.

Each subject made 100 judgments on each card of each series, of 3500 judgments in all, so that the total number of judgments for all subjects was 42,000. These experiments were made during different periods varying for different subjects from five to seven weeks. As explained above, subjects were instructed to sit facing the opening in the screen and to assume the best position for distinct vision. They were to look first at the left-hand line or standard stimulus and then at the right-hand line or comparison stimulus and then to give the judgment "longer", "equal", or "shorter". No attempt was made to get any record of the subject's feeling of certainty in regard to these judgments nor were any "guesses" recorded in the case of the pairs judged equal. As the feeling of certainty is bound to be more or less a relative thing when the subject comes to express it in words, and as to require an introspective judgment on a judgment tends to distract attention from the primary judgment which is the object of the experiment, it did not seem wise to pay attention to the matter of certainty. The subjects were told to try hard to see whether the right-hand line was longer or shorter, but failing to see that it was either, to give the judgment "equal". This idea of subjective equality was explained and insisted on. It is true that some observers consider that there is a distinction between inability to see which is longer of two lines (doubtful case) and the judgment "equal" which is a category correspond-

ing to "longer" or "shorter" when given with some feeling of certainty. It seemed best to make no distinction between these two classes since it seems to the experimenter that the "doubtful cases" and the equality judgments are both equality cases in that the observer fails to perceive the difference between two stimuli which are presented to him. When observers speak of a difference of which they cannot determine the direction, as when two lines do not appear to be the same length and yet the observer is unable to tell which seems longer, it seems probable that there is some fluctuation in the "sensation of length" due to variations in objective or subjective conditions. For example, the influence of immediate fixation may be felt. A subject might look at the standard line and then at the comparison line. The latter might seem slightly longer but so uncertainly that the subject's eyes would return to the standard which when fixated might itself seem the longer of the two; that is, each line in its turn seems longer than the other. This is a true "doubtful case". From the standpoint of this experiment, however, it seemed best to insist on the definition of subjective equality given above.

While all subjects apparently tried to use the equality judgment as directed, there was much variation in its actual use. It would seem that this variation was greater than would be accounted for by subjective differences in different subjects and yet an effort was made to keep outside conditions in the way of directions, explanations, etc., the same for all. Seventeen per cent of all judgments given were of the equality type as defined above. These varied from less than 4 per cent in the case of one subject to 29 per cent in another.<sup>2</sup> There was also a great variation in the distribution of these judgments among the different pairs and in the different series. Subject X gave 264 equality judgments but 155 of these were in the first series and Subject VI gave 1022, with 536 in his first two series. These variations suggest that a series might be given in which a subject

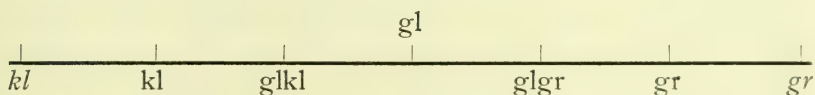
<sup>2</sup>Urban's subjects gave 14 per cent. of all judgments as "equal" with a range of 5 per cent. to 33 per cent. His subjects show less variation from series to series. This is certainly one of the advantages of training one's subjects.

might make the judgment "longer" or "not longer" on 100 pairs of comparison stimuli, then the judgment "equal" or "not equal" on the next 100 pairs, and on the third hundred the judgment "shorter" or "not shorter". The difficulty of giving uniform directions to inexperienced subjects would be lessened and the choice between two judgments is of course simpler than among three for either trained or untrained subjects.

Compare this simplicity with the complications introduced by requiring "guesses" and introspections as to "degrees of certainty" on primary judgments. Take for example the series sometimes used:

"smaller"	(certain)
"smaller"	(less certain)
"doubtful"	(probably smaller = "guess")
"equal"	(certain)
"doubtful"	(probably greater = "guess")
"greater"	(less certain)
"greater"	(certain) <sup>3</sup>

Instead we might almost give each of these seven judgments a number corresponding to the seven values of our comparison stimuli taken in order and then ask the subject to call or try to call each comparison line as it is presented by its appropriate number. If it was "certainly shorter" he would say "144", if "less certainly", "146", if "doubtful but probably shorter", he would say "148", etc. Or a subject might adopt the visual scheme mentioned by Martin and Müeller.



In this scale zero stands at the middle and corresponds to subjective equality. To the left are the smaller judgments and to the right the greater. The distance from the center is a measure of the certainty of a judgment in any particular part of this scale.

<sup>3</sup> Compare Urban's  $h_1$ ,  $h_2$ , etc.

Thus *kl* is "certainly smaller", *kl* is "smaller", *gkl* is "equal, probably smaller", etc. The subject gives his judgment a place in this visual scale and then gives the appropriate answer.<sup>4</sup> But either this plan or the previous one involves complications in judgment where simplicity is desirable. Urban well says (*op. cit.* p. 15) "that there is some danger of mixing up two different problems, the problem of the degree of subjective confidence and that of the accuracy of sensation".

Subjects were encouraged to give judgments promptly but not hurriedly. They were told to avoid as far as possible the inevitable tendency to compare the comparison line on which judgment was being made with the previous comparison line which had occupied the same position in the field of vision rather than with the accompanying standard line.<sup>5</sup> Although the subjects were told to view the standard line first and the comparison line afterward, there was also a tendency to let the eyes pass back and forth from one to the other. In the long run the influence of the previous comparison line in the judgment of any pair of lines is probably appreciable, but not serious in the case of subjects who are conscientiously trying to carry out the plan of the experiment if care is taken to change the serial order often enough so that this influence may not always be in the same direction. As brought out later, it is probable in this experiment that it would have been better to have used ten rather than five serial orders. The following figures show that in cases where the objective difference of a pair of comparison lines is small this influence is likely to be effective. In series III comparison line 150 follows comparison line 156, while in series V 150 follows

<sup>4</sup> Zur Analyse der Unterschiedsempfindlichkeit, L. J. Martin und G. E. Müller, Leipzig, 1899; p. 50. Dr. Henri, the subject who used this scale, said "Ganz unwillkürlich hat sich bei mir ein visuelles Schema für die Antworten ausgebildet. Das Schema besteht aus einer Geraden. In der Mitte ist Null, rechts die Antworten 'grösser', links die Antworten 'kleiner'. Manchmal, nachdem Ich das zweite Gewicht gehoben habe, stelle Ich nur visuell die Antwort vor und nur etwas später kommt die Antwort ins Bewusstsein. Dieses scheint meistens in den Fällen *gkl* und *glgr* zu geschehen."

<sup>5</sup> Nebenvergleichen.

146. Taking all twelve subjects into account, the results are as follows:

Comparison line 150 compared with standard line 150 was judged			
	shorter or longer;	shorter;	longer;
In Series III.....	980	564	416
In Series V.....	984	488	496

It will be observed that the number of equality judgments (220 in series III and 216 in series V) remains practically the same while the change in the other two classes of judgments is apparent. This comparison suggests that the previous appearance of line 156 makes a subject more likely to underestimate line 150, while the previous appearance of line 144 or 146 makes a subject more likely to overestimate this same line.

Several subjects spoke of the influence in some cases of an absolute impression. A certain line, either standard or comparison, would seem "short" or "long" by itself alone and that without conscious reference to the accompanying or preceding lines. Again a pair of lines would seem "short" and in the case of one subject a whole series seemed "shorter than any previous series". These absolute impressions are a factor in the formation of our judgments and in some cases must exert an appreciable influence.

One revolution of the drum required about 18 seconds. Each card was presented for about  $1\frac{1}{2}$  seconds and the change to the next required 1 second. This change was rapid enough to make one pair of lines seem to disappear at once; then there was a blank; and then a second pair appeared. There was little to suggest a moving stimulus though, of course, the lines did move in the field of vision 1 inch before and 1 inch after they were at rest. The simplicity of the apparatus commends itself in that neither experimenter nor subject need give attention to its manipulation. A detailed description follows on page 41.

Every effort was made to avoid illusions of any kind and, where time and space errors could not be avoided, to keep these as constant as possible. To this end the lines were placed in the horizontal plane and moved downward to be replaced by the next pair. Each line was at the same distance from the observer as the

others. No effort was made to have the subjects remember the standard from one card to another. Subjects were not even asked to remember that, objectively speaking, the standard lines were all the same. In other words, each pair was to be judged for itself as it came along.

At least two other procedures might obviously have been pursued. The standard line might have been put at random, now at the right, now at the left. The subject would then have been asked to pick out the longer of each pair. Again the standard line might have been placed at the right throughout the experiment, reversing our actual procedure. The former change would have made it more difficult to summarize the records accurately and it would have been less feasible to have subjects act in turn as recorders. The left-to-right method was preferred to the reverse in that the left-to-right excursion of the eyes is the customary one in reading.

No attempt was made to emphasize *rightness* or *wrongness* of judgments except as noted above in the matter of securing attention. Occasionally during times of rest, a subject would speak of a series as "difficult" or "easier than the previous series" but the correctness of his answers when finally counted seldom bore him out in his idea that he was doing better. If a subject knows that he is dealing with seven pairs only one of which has the quality of objective equality, he is very likely to be influenced by his knowledge the moment emphasis is put on the correctness of his judgments. Nor is this matter of correctness of particular interest to the experimenter except as it gives evidence of some constant "error" of observation. The experimenter is much more concerned with his subject's ability to report accurately and promptly subjective equality and subjective difference. This again suggests the advantage of having trained subjects. On the other hand, if psychophysical methods are ever to find extensive clinical or laboratory use, it will be necessary to devise methods of testing untrained subjects and either to record many experiments in a short time or be satisfied with few.

## ABSOLUTE FREQUENCY

The record-sheets of each subject were collected and the number of each kind of judgments made upon each comparison line in each half series (50) was found and marked on the record-sheets. The results of these countings are the absolute frequencies of the different classes of judgments for each comparison stimulus and are tabulated on pages 50 to 53. There are considerable variations in the different series and some even in the two fifties (a and b) of a single series. Since other conditions were kept as nearly constant as possible, these variations are in many cases probably due to some influence of the order in which the pairs were presented in the different series. As stated above, there seems to be a tendency to compare a comparison line with the preceding comparison line rather than with the accompanying standard line. It is therefore of interest, in comparing the different series of a single subject or in comparing the judgments of different subjects one with another, to form some estimate of the extent to which the conditions under which these data have been derived have been kept constant. If we make  $s$  observations in each of  $n$  series of a certain event  $E$  of which  $m_1, m_2, m_3, \dots, m_n$  have given a certain result, the relative frequencies of these results will be represented by the quotients

$$\frac{m_1}{s}, \frac{m_2}{s}, \frac{m_3}{s}, \dots, \frac{m_n}{s}.$$

If the sum of these ratios be divided by  $n$  we derive  $a$ , the arithmetical mean; that is

$$a = \frac{\sum m_i}{ns}$$

If a large number of observations are made, this arithmetical mean may be taken as the mathematical probability of the event  $E$ . As for example, if successive drawings are made from the same urn containing a certain number of black and white balls, and the balls are returned after each drawing, the relative frequencies of a given result will be grouped around a certain value  $p$  with a

coefficient of precision  $h$  which may be obtained from the equation

$$h = \sqrt{\frac{s}{2p(1-p)}}$$

according to the theorem of Bernouilli. In the same way we may expect, under constant conditions in the collection of experimental data, a coefficient of precision

$$h = \sqrt{\frac{s}{2a(1-a)}}$$

If we treat our results  $m_1, m_2, m_3, \dots, m_n$  by the method of least squares, we may compute a coefficient of precision  $h'$  according to the following formula:

$$h' = \sqrt{\frac{n-1}{2 \sum \left( \frac{m_i}{s} - a \right)^2}}$$

A comparison of the two values,  $h$  and  $h'$ , gives an idea of the extent to which the conditions of the experiment have remained constant, since in that case the ratio  $\frac{h}{h'}$  approximates 1. This ratio is called the *coefficient of divergence* and we have

$$Q = \sqrt{\frac{\sum \left( \frac{m_i}{s} - a \right)^2 s}{a(1-a)(n-1)}}$$

In the present experiment, this calculation was made for all twelve subjects with reference to the distribution of the relative frequencies for shorter, equal, and longer judgments. The results appear on page 55. Since  $n = 10$  and  $s = 50$  for all twelve subjects, we have the following convenient arrangement for this formula, for the values  $\frac{m_i}{100}$ , etc., and  $\frac{a}{2}$  may be obtained from the tables of absolute frequencies by a mere adjustment of decimal points.

$$Q = \sqrt{\frac{200 \sum \left( \frac{m_i}{100} - \frac{a}{2} \right)^2}{9a(1-a)}}$$

It is of interest to study the values thus obtained. If we average the coefficients of divergence for all twelve subjects, we obtain the following results:

c.s.	shorter	*	equal	*	longer	*
144 .....	2.09	+ 0.25	1.96	— 0.25	1.54	— 0.47
146 .....	2.05	+ 0.21	2.05	— 0.16	1.84	— 0.17
148 .....	2.33	+ 0.49	2.50	+ 0.29	1.94	— 0.07
150 .....	1.82	— 0.02	2.24	+ 0.03	2.26	+ 0.25
152 .....	1.64	— 0.20	2.42	+ 0.21	2.37	+ 0.36
154 .....	1.43	— 0.41	2.14	— 0.07	1.99	— 0.02
156 .....	1.50	— 0.34	2.14	— 0.07	2.15	+ 0.14
Mean .....	1.84		2.21		2.01	

We may observe that, from the standpoint of a "normal" distribution, these coefficients are all large. This is not surprising when we consider that none of the subjects was trained in this particular experiment, and that six of the twelve had had no training in similar experimentation. As we might expect from our observations above on the distribution of the equality judgments, the coefficients for these judgments give a mean (2.21) clearly larger than that for either shorter or longer judgments (1.84 and 2.01 respectively). It will be observed that the coefficients for shorter judgments are largest for the shorter comparison stimuli, 144, 146, and 148; those for equality judgments are largest for comparison stimuli in the middle of our series, 148, 150, and 152; while those for longer judgments are largest for the latter part of the series, 150, 152, 154, and 156.

In this connection it is of interest to compare the coefficients of different subjects. It will be remembered that the six subjects who were employed were not only less experienced but also younger and less mature than the other six. If we arrange the six inexperienced subjects in one table and the other six in another we have the following result:

(\* Differences from mean for all seven comparison stimuli.)

(A) Six inexperienced subjects				(B) Six experienced subjects			
	s	e	l		s	e	l
I.....	2.14	3.27	2.09	II.....	1.45	2.37	2.46
III.....	2.75	1.99	2.53	VI.....	2.19	2.02	2.55
IV.....	2.19	2.93	2.60	VII.....	1.38	1.41	1.25
V.....	1.75	2.40	2.27	IX.....	1.59	1.73	1.31
VIII.....	2.21	1.94	1.82	XI.....	1.37	1.31	1.38
X.....	1.68	3.27	1.97	XII.....	1.37	1.77	1.77
mean .....	2.12	2.63	2.21	mean.....	1.56	1.77	1.79
		2.32				1.70	

It may be noticed at once that the several values and averages of the coefficients of divergence for Group A are larger than those for Group B and that the greatest differences and greatest lack of uniformity are in the equality column. The inexperience of the members of Group B may in all probability account for the higher values and lack of uniformity shown. To study this further it has been interesting to study Subject I of Group A. Coefficients of divergence were calculated for the last 2,800 of his 3,500 judgments (*i.e.* Series II, III, IV, and V) and compared with those of the entire 3,500. We find the figures to be as follows:

For entire 3,500		shorter	For last 2,800	
mean 2.14 {	2.81	144	1.56	} mean 1.65
	3.19	146	1.34	
	2.25	148	2.07	
	2.28	150	2.01	
	1.82	152	1.79	
	1.27	154	1.32	
	1.37	156	1.49	
equal				
mean 3.27 {	3.28	144	1.81	} mean 2.09
	3.77	146	1.44	
	3.16	148	2.06	
	3.32	150	2.46	
	3.83	152	2.98	
	3.21	154	2.53	
	2.32	156	1.32	
longer				
mean 2.09 {	1.15	144	1.04	} mean 1.76
	1.72	146	1.62	
	2.23	148	2.10	
	1.69	150	1.05	
	3.15	152	2.71	
	2.35	154	2.08	
	1.48	156	1.74	
mean 2.50		1.83		

These figures plainly show that the conditions under which the last 400 judgments on each comparison pair were made were more nearly constant than for the entire experiment. Subjects III, V, and X from Group A show the same tendency to "steady down" after the first series.

If we arrange our twelve subjects according to the mean value of the coefficients of divergence for all three classes of judgments we have the following result:

1) VII.....	1.34	7) V.....	2.14
2) XI.....	1.35	8) VI.....	2.25
3) IX.....	1.54	9) X.....	2.31
4) XII.....	1.62	10) III.....	2.42
5) VIII.....	1.99	11) I.....	2.50
6) II.....	2.09	12) IV.....	2.57

Out of the first six, five are from Group B. On the basis of his last four series Subject I moves up from eleventh to fifth place. This bears out our previous conclusion that experience is an important factor in experimentation of this kind. This should not imply the element of practice in the usual sense of the word, *i.e.* that, after making many judgments, Subject I could judge more "accurately" of the objective relations of the comparison lines. Such use of the word would imply that, making a mistake, the subject was told of it and so was less likely to repeat his error. It is rather practice in reporting promptly and accurately the subject's own (subjective) judgment. The subjects of Group B on the whole did this better from the first than those of Group A.

This point is illustrated in the case of subject VII who judged comparison line 156 longer than the standard (150) only 340 out of 500 times but with a coefficient of divergence of 1.00. This was not accurate from an objective standpoint but represents good ability in conforming to the conditions of the experiment in a uniform manner and a prompt and accurate introspection as to the subjective state ensuing upon an observation.

The high values of the coefficients of divergence for all twelve subjects for all three classes of judgments suggest that the change of condition due to the order of the comparison pairs in the different series has been effective and that it would have been

better to have used more arrangements, perhaps ten instead of five. The causes of variation for one pair could then be made more nearly the same as for any other. As it is in the doubtful cases that the serial order would be most likely to produce an effect, it is not strange that the averages of the coefficients of divergence of all subjects on the different comparison pairs are largest for comparison lines 152, 150, and 148 as shown in the following table.

c.s.	
144	1.86
146	1.96
148	2.26
150	2.11
152	2.14
154	1.85
156	1.93

On the other hand, if we had taken pains to reduce these large coefficients by practice, or if the serial order had been subject to frequent change, we should have failed, very likely, to have had any evidence other than *a priori* on the advantage of training.

## RELATIVE FREQUENCY

Having tabulated the absolute frequencies of the different kinds of judgments for the different subjects, we may proceed to determine the relative frequencies of these judgments. Since 500 judgments were made on each comparison line, these frequencies may be found in each case by multiplying the absolute frequencies by 0.002. The results appear on pages 54 and 55 opposite the numbers 144, 146, etc. These may be considered as empirical determinations of the values of the psychometric functions. Intermediate values of these functions may be interpolated by Lagrange's formula.

This interpolation is based on the assumption that the relative frequency or probability of any judgment is an algebraic function of the objective relations of the comparison stimuli used in an experiment of this nature. If a graded series of compari-

son stimuli  $x_1, x_2, x_3, \dots, x_n$  give relative frequencies  $p_1, p_2, p_3, \dots, p_n$ , we may assume that  $p = f(x)$  between the limits within which  $x_1 > x < x_n$ . Then by Lagrange's formula we have

$$\begin{aligned}
 p = & \frac{(x-x_2)(x-x_3)\dots(x-x_n)}{(x_1-x_2)(x_1-x_3)\dots(x_1-x_n)} p_1 \\
 & + \frac{(x-x_1)(x-x_3)\dots(x-x_n)}{(x_2-x_1)(x_2-x_3)\dots(x_2-x_n)} p_2 + \dots \\
 & \dots + \frac{(x-x_1)(x-x_2)\dots(x-x_{n-1})}{(x_n-x_1)(x_n-x_2)\dots(x_n-x_{n-1})} p_n
 \end{aligned}$$

In the present experiment, this formula gives us the psychometric function  $p$  for any comparison stimulus  $x$  between the limits within which  $144 < x < 156$ . Then on the basis of our empirical data,  $p_1, p_2$ , etc., we have

$$p = \frac{(x-146)(x-148)(x-150)(x-152)(x-154)(x-156)}{2 \cdot 4 \cdot 6 \cdot 8 \cdot 10 \cdot 12} p_1$$

$$- \frac{(x-144)(x-148)(x-150)(x-152)(x-154)(x-156)}{2 \cdot 2 \cdot 4 \cdot 6 \cdot 8 \cdot 10} p_2$$

$$+ \frac{(x-144)(x-146)(x-150)(x-152)(x-154)(x-156)}{4 \cdot 2 \cdot 2 \cdot 4 \cdot 6 \cdot 8} p_3$$

$$- \frac{(x-144)(x-146)(x-148)(x-152)(x-154)(x-156)}{6 \cdot 4 \cdot 2 \cdot 2 \cdot 4 \cdot 6} p_4$$

$$\begin{aligned}
 & + \frac{(x-144)(x-146)(x-148)(x-150)(x-154)(x-156)}{8 \cdot 6 \cdot 4 \cdot 2 \cdot 2 \cdot 4} p_5 \\
 & - \frac{(x-144)(x-146)(x-148)(x-150)(x-152)(x-156)}{10 \cdot 8 \cdot 6 \cdot 4 \cdot 2 \cdot 2} p_6 \\
 & + \frac{(x-144)(x-146)(x-148)(x-150)(x-152)(x-154)}{12 \cdot 10 \cdot 8 \cdot 6 \cdot 4 \cdot 2} p_7
 \end{aligned}$$

Putting  $c_1, c_2, c_3, \dots, c_7$  for these fractional coefficients, we have

$$p = c_1 p_1 + c_2 p_2 + \dots + c_7 p_7.$$

Since these coefficients are the same for all subjects, we find that the logarithmic calculation for interpolation for the values  $x = 145, x = 147$ , etc., is facilitated by first calculating  $\log c_1, \log c_2, \dots$  and  $\log c_7$  for each value of  $x$  for which it is desired to interpolate. These may then be combined with  $\log p_1, \log p_2$ , etc., which are different for different subjects. The fact that the sum of the values of the three psychometric functions corresponding to the judgments "longer", "equal", and "shorter" for any comparison stimulus  $x$  is equal to 1 serves as a convenient check for the values obtained.

The results of these interpolations appear in the same tables with the observed relative frequencies on pages 54 to 55 opposite the numbers 145, 147, 149, 151, 153, and 155. In most cases the interpolated values are intermediate between adjacent empirical values of the psychometric function. In some cases this is not true and we have fluctuating values corresponding to successive comparison stimuli. These are usually near one or the other extremes of our series of comparison pairs and are most marked where  $x = 145$  and  $x = 155$ . In five of the subjects (I, IV, VII, VIII, and XII) one of the interpolated values of the psycho-

metric function for the comparison stimuli 145 or 155 is either greater than unity or less than zero. These values can not, of course, be considered as probabilities. They show rather that the conditions at the extremes of our series are not the same as in its middle course. This corresponds with the evidence of introspection and with our observations upon the coefficients of divergence.

This difference between the conditions of judgment on the extremes of a series of comparison stimuli and on those stimuli more nearly equal to the standard has been pointed out by Urban in connection with his experiment with lifted weights.<sup>1</sup> He formed the mean of the coefficients of divergence for each of his seven subjects on the different comparison weights and for the three classes of judgments and noted that in all three classes the values of the coefficients of divergence were larger for the middle comparison weights of his series. If we do the same for the five subjects mentioned above, we arrive at the following result:

Mean values of coefficients of divergence for subjects I, IV, VII, VIII, and XII.

c. s.	shorter	equal	longer
144 .....	1.76	1.94	1.25
146 .....	2.18	2.43	1.43
148 .....	2.48	2.81	1.65
150 .....	2.12	2.36	2.45
152 .....	1.79	2.36	2.62
154 .....	1.34	2.01	1.92
156 .....	1.31	1.95	1.87

These also give evidence that, where the difference between comparison stimulus and standard is small, the conditions of judgment are not the same as at the extremes of a series where the differences are greater.

A study of the psychometric functions themselves is of interest. That for shorter judgments has a value of about 0.85 for comparison line 144 and, for most subjects, becomes gradually smaller

<sup>1</sup>Die Psychophysischen Massmethoden als Grundlagen empirischen Messungen, F. M. Urban, Ph.D., 1909; p. 280.

throughout the series, being less than 0.10 for comparison line 156. The psychometric function for longer judgments begins at some value less than 0.10 for most subjects and increases throughout the series, gaining for comparison line 156 a mean value of 0.82. These two series of values are more or less symmetrical, one increasing as the other decreases. The psychometric functions for equality judgments vary greatly among the different subjects and will be discussed more at length by themselves. In general they have values nearly equal to zero for comparison lines at the extremes of our series and attain their maximum values near the middle of the series.

A glance at these tables of the numbers of relative frequency will show that, for each subject, for values of  $x$  up to a certain point, there is a probability greater than 0.5 that the judgment "shorter" will be given and that, beyond a certain value of  $x$ , there is a probability greater than 0.5 that the judgment "longer" will be given. For values of  $x$  within this interval there is no probability greater than 0.5 that a comparison stimulus will be judged either shorter or longer than the standard. This is the *interval of uncertainty*. It corresponds to the "difference threshold" of the gradation methods.

Since the values of the psychometric functions increase or decrease with considerable regularity near the middle of our series, we may calculate the limits of this interval of uncertainty by interpolation as of linear functions. For this purpose we find in the tables the values of  $x$  corresponding to the values of  $p$  which are nearest above and nearest below 0.5 respectively and compute an intermediate value of  $x$  for which  $p = 0.5$  for shorter judgments. The process is then repeated for longer judgments and the difference between the two results gives the interval of uncertainty in the same units as we measure the comparison stimuli.

Thus if  $x_a$  gives probability  $p_a$ , and  $x_b$  gives probability  $p_b$ ,

where  $p_a > 0.5 > p_b$ ,

$$\text{let } m = \frac{(x_b - x_a) (p_a - 0.5)}{(p_a - p_b)}$$

and from the equation  $x = (x_a + m)$  we derive the value  $x$  of the comparison stimulus for which  $p = 0.5$ . The results of these calculations of the intervals of uncertainty for the various subjects appear on page 56. The position of this interval with reference to a comparison stimulus of 150, the position of objective equality in our series, is of interest as evidencing a tendency in different subjects to under- or over-estimate the comparison stimulus. The values of the stimulus corresponding to the middle point of this interval are therefore given in the table. It will be noted that the values range from 148.80 (Subject II) to 152.09 (Subject III) but there seems to be no uniform tendency to under- or over-estimation since the mean value for all twelve subjects is 150.24, six being above and six below the mean. Urban's subjects judged a series of weights in comparison with a standard weight of 100 grams. Averaging similar computations for his seven subjects, we find that the middle point of the interval of uncertainty corresponds to a comparison weight of 97.56 grams, showing a decided tendency to overestimate the second (comparison) weight lifted.

## GRAPHS

A study of the course of the psychometric functions for the three classes of judgments between the limits of the extremes of our series of comparison stimuli is facilitated by the use of graphs to represent these values. These were plotted and appear on pages 43 to 48. It will be seen that the values of the comparison lines 144, 146, etc., are laid off on the x-axis, and on the y-axis we have a scale of percentages from zero on the x-axis to the upper margin which represents the unit. The ordinates for  $x = 144$ ,  $x = 146$ , etc., therefore represent the relative frequencies of the three classes of judgments made upon these comparison stimuli. The general type of these curves is characteristic of the psychometric functions when the comparison stimuli used vary but little from the standard stimulus. If a subject's judgments correspond in every case to the objective

relations between the comparison stimuli and the comparison stimulus, the psychometric function for shorter judgments would have the value 1 for all comparison stimuli less than the standard, would drop to zero for a comparison stimulus equal to the standard and would remain at zero for all greater values of the comparison stimulus. Conversely, the psychometric function for longer judgments would have the value zero for all comparison stimuli less than or equal to the standard and would be equal to 1 for all greater comparison stimuli. The probability of an equality judgment would be equal to 1 for a comparison stimulus equal to the standard but for all greater or less comparison stimuli would be equal to zero. This condition of affairs would be approximated if judgments were made on comparison stimuli differing from each other by an amount greater than the absolute threshold of difference: that is, a difference that was always perceived. Where the difference between the comparison stimuli themselves and between the comparison stimuli and the standard are small, the course of the psychometric functions will be much as in these charts. The psychometric function for shorter judgments begins with large values for comparison stimuli less than the standard and rapidly approaches zero for comparison stimuli at the other end of our series. That for longer judgments begins with small values for comparison stimuli shorter than the standard and approaches 1 for values greater than the standard. The psychometric function for equality judgments is represented by a graph which is more or less arched, reaching its maximum for a comparison stimulus nearly equal to the standard and assuming values nearly equal to zero at either end of our series.

It will appear, as previously noticed, that the interpolated values of the psychometric functions for comparison stimuli 145 and 155 are in some cases less than zero and in others greater than 1. Within the limits 146 and 154 for the comparison stimuli of our series there is an approximation of a type: that is, although there are marked differences in the courses of the graphs for different subjects, there is also a marked similarity in the general trend of the curves. This similarity is still more marked if we consider

only the functions for shorter and longer judgments and if we center our attention upon the part of the charts where these two cross one another rather than along the ordinate where  $x = 150$ . The greatest differences are noticed in the graphs for equality judgments and in the lengths and positions of the different intervals of uncertainty.

The interval of uncertainty may be found in the following manner. A straight line is drawn parallel to the base-line or x-axis of one of these charts midway between the upper and lower margins. Since  $p = 0.5$  for all points on this line, the intercept lying between the points of intersection with the graphs for shorter and longer judgments respectively is the interval of uncertainty. The points of intersection, by reference to the x-axis, give the values of the thresholds in the direction of increase and of decrease.

As for the equality judgments, it is evident that there is great diversity among subjects both in the number of such judgments (shown roughly by the area enclosed below the equality graph) and also in their distribution (shown by the course of the graph, the position of its crest, etc.). It will be noticed that in only a single case (Subject XII) does the function for equality judgments exceed 0.5. For three of the subjects, however (Subject VI on 148, Subject VII on 151 and 152, and Subject XI on 150, 151, and 152) certain values of the equality function are greater than those for either longer or shorter judgments on the same comparison pairs so that, although the probability that a judgment "equal" will be given does not exceed the probability that such a judgment will not be given, there are still "odds" in favor of an equality judgment as against either a longer or a shorter judgment.

## THE METHOD OF JUST PERCEPTIBLE DIFFERENCES

Many mental states may be compared quantitatively. A certain sound-sensation is more intense than another but less intense

than a third. Two different reds do not necessarily produce the same sensations of either color or brightness. One weight "feels" heavier than another. Since in all these cases the objective stimuli also vary, our everyday language for describing such mental states hopelessly confuses their subjective relations with the objective relations of the stimuli. Thus to say that "one sound is louder than another" may refer to either sensation or stimulus or to both. If we ask whether the ultra-violet rays of the spectrum are light or not, we find that we must first define whether by light we mean a form of wave-motion having certain limits of wave-length, or whether we mean a form of wave motion capable of exciting the retina. Not only may mental states such as sensations, space-perceptions, etc., be compared when it is easy to recognize that they are different, but it becomes interesting to compare two which are so nearly alike as to be almost indistinguishable since, though we recognize that they are not the same, we fail to see in what direction the difference lies. Again we may perceive no difference at all: that is, if they are subjectively equal. This does not mean that the stimuli immediately producing these subjectively equal mental states are themselves necessarily equal or nearly so. Contrast, fatigue, association, etc., may be factors in the resulting mental state so that two stimuli which are objectively equal may produce sensations or perceptions which are quite different or two very different stimuli may result in mental states which are subjectively equal. Any of the various kinds of space illusion in the comparison of the lengths of two straight lines is an example in point. Two lines may be exactly equal and yet because one is drawn in a vertical position it may "seem" considerably longer than the other drawn in a horizontal position. On the other hand, we may either shorten this vertical line or lengthen the other until they seem to be the same length.

When an observer fails to perceive any difference between two such mental states, he gives the judgment "equal" although from the difference in physical stimulation we might infer a slight difference in the mental states produced. This non-perceptible difference in sensation seems like a contradiction of terms and

yet may be explained in the following way.<sup>1</sup> Suppose a graded series of stimuli  $R_1, R_2, R_3, \dots R_n$  so little different from one another that the sensation produced by  $R_2$  could not be distinguished from that produced by  $R_1$ , nor that by  $R_3$  from that by  $R_2$ , and so on, and still the sensation produced by  $R_7$  might be easily distinguished from that given by  $R_1$ . Moreover, though a subject judging such a series would judge  $R_1 = R_2, R_2 = R_3, R_3 = R_4, \dots R_{n-1} = R_n$ , this is in no sense the same as judging  $R_1 = R_n$ . Whether we accept this reasoning as valid or not, and whether a non-perceptible difference in sensation is a contradiction of terms or not, it is self-evident that there is a non-perceptible difference in stimulation and probably in cerebral excitation.

It is the purpose of the point of view in experimentation which has come to be called *The Method of Just Perceptible Differences* to study the range of stimulation within which a subject "perceives no difference" in the resulting sensations and beyond which a difference is "perceptible". Given a series of stimuli as above,  $R_1, R_2$ , etc., producing sensations or mental states  $S_1, S_2, \dots S_n$  such that to the observer  $S_1$  is "indistinguishable" from  $S_2, S_2$  from  $S_3$ , etc., we may explore this range, we are told, in the following manner. Since  $S_7$  is indistinguishable from  $S_6$  and  $S_8$ , we may compare it further with  $S_5$  and  $S_9, S_4$  and  $S_{10}$ , etc. We shall find all sensations, we are further told, within a certain range, say  $S_5$  to  $S_{10}$ , indistinguishable from our standard  $S_7$ . We then consider  $R_5$  and  $R_{10}$ , the stimuli corresponding to the limits of this interval, as significant for our experiment and name  $R_5$  the *just imperceptible negative difference* and  $R_{10}$  the *just imperceptible positive difference*. We may then compare  $S_7$  with sensations outside of this range. Beginning, for example, with  $S_1, S_2$ , etc., we find  $S_4$  the last to be less than  $S_7$ , and beginning higher in our series and coming back-

<sup>1</sup> This argument is quoted from Prof. Karl Stumpf's *Tonpsychologie* by G. F. Stout in his *Manual of Psychology* (1899), pp. 120 *et seq.* It seems to the writer that "mere sensation", "sensation", "sense-experience", and "sensory element" are unnecessary elaborations of *sensation* as this term is usually used.

ward, we may find  $S_{12}$  to be the last greater than  $S_7$ , and we have two more significant values,— $R_4$ , the *just perceptible negative difference*, and  $R_{12}$ , the *just perceptible positive difference*. The mean of  $R_4$  and  $R_5$  is the *threshold in the direction of decrease*, and the mean of  $R_{10}$  and  $R_{12}$  is the *threshold in the direction of increase*. The difference between these two means gives an *interval of uncertainty or difference threshold*. All sensations caused by stimuli lying between

$$\frac{R_4 + R_5}{2} \text{ and } \frac{R_{10} + R_{12}}{2}$$

are therefore presumably “indistinguishable” from  $S_7$ .

But this “method” is not so simple as we are asked to believe. What do we mean when we say that  $S_7$  and  $S_8$  are “indistinguishable”? Do we mean *every* time that  $R_7$  and  $R_8$  are judged by our subject; or at a single trial; or in a majority of instances? For though  $R_7$  and  $R_8$  remain constant,  $S_7$  and  $S_8$  do not necessarily do so, owing to variation in attention, fatigue, practice, expectation, etc. We may vary expectation, for example, by presenting to a subject  $R_7$  with  $R_1$ ,  $R_2$ , and  $R_3$  in serial order at one time and at another in a mixed order unknown to the subject (*method of irregular variation*). Time and space errors enter into these mental states also. Even where sources of variation are eliminated as far as possible or where every effort is made to allow for them, we still find variations in our results. Two comparison stimuli are not always judged in the same way. We have at different times  $S_7 < S_8$ ,  $S_7 = S_8$ , and  $S_7 > S_8$ . When this difficulty arises, we may proceed to make a great number of observations on these threshold values but they are not mathematical constants and we must define them. We may define the threshold in the direction of increase as the least value of the comparison stimulus which is *always* judged greater than our standard. This would be an absolute standard but would not help us in studying our subject’s sensitivity in the discrimination of small differences. Again we may consider each observation upon one of these four “differences” as equally valid with any other and take the mean

values of the four as determinations of the "just perceptible" and "just imperceptible differences". This allows for any of the variations mentioned above in that they will at times be operative in one direction and again in another. It does not make allowance for disturbing factors which always act in the same direction. It is this treatment of results that is usually employed in the method of just perceptible differences. In this discussion the thresholds and intervals of uncertainty, or difference thresholds, where this definition is applied will be spoken of as "empirical" or "observed" thresholds, etc., by the method of just perceptible differences.

If we set aside the idea of an "absolute threshold" we may arbitrarily define the threshold in the direction of increase as the least value of the comparison stimulus which is judged greater 50, 60, or 75 per cent of the time. This has the advantage of giving us a definition as soon as we may agree upon any particular percentage. It also serves as a connecting link between the error and gradation methods.

That these various questions arise will make it plain that *the method of just perceptible differences* is not a form of experimental procedure.<sup>2</sup> Laboratory methods may be varied considerably as long as they furnish data upon the subjective relations which hold between  $S_7$ , the result of our standard stimulus  $R_7$ , and other mental states not very different from this which are themselves the results of our comparison series of stimuli,  $R_1$ ,  $R_2$ , etc. The *method of just perceptible differences* is rather a psychophysical point of view which determines the treatment of experimental data. It may make its claim on any of the "gradation methods" to furnish these data, but the laboratory procedure will depend upon the nature of the sense-organ or other body structures involved, the limitations of the apparatus used, and many other technical and practical considerations.

It will be observed that judgments upon a series of comparison stimuli, some greater and some less than the standard stimulus, arranged in varying order such as we find in this experiment, lend

<sup>2</sup> E. B. Holt, The Classification of Psychophysics Methods; *Psychological Review*, Vol. XI, 1904.

themselves readily to treatment by the method of just perceptible differences. Suppose that a series of our comparison stimuli were judged as follows:

156 "longer"  
 148 "equal"  
 146 "shorter"  
 152 "equal"  
 144 "shorter"  
 154 "longer"  
 150 "longer"

We may then proceed to make an empirical determination of the values of the thresholds in the following manner. We find 150 the shortest comparison stimulus which is judged longer. This is the just perceptible positive difference. We find 152 the longest comparison stimulus on which the judgment "longer" is not given. This is the just imperceptible positive difference. The threshold in the direction of increase is therefore the mean of these two values, or 151. We find further that 146 is the longest comparison stimulus which is judged shorter (the just perceptible negative difference) and that 148 is the shortest comparison stimulus on which the judgment "shorter" is not given (the just imperceptible negative difference). The threshold in the direction of decrease is therefore 147. Subtracting 147 from 151, we have the difference threshold or interval of uncertainty equal to 4. This empirical determination of the difference threshold becomes more trustworthy if we average the results of a large number of series of judgments upon our different comparison pairs.

Since the relative frequency of a certain judgment upon any comparison stimulus may be considered the probability that such a judgment will be given, we may again represent this probability by  $p$  and the probability that this judgment will not be given will be  $(1 - p)$ . Given  $p_1, p_2, p_3, \dots, p_7$  as the probabilities of a longer judgment on comparison stimuli  $x_1, x_2, x_3, \dots, x_7$ , we have as probabilities that an equal or shorter judgment will be given on the same comparison stimuli  $(1 - p_1), (1 - p_2), \dots, (1 - p_7)$ . We may then set out to calculate

the probability  $P_n$  that any comparison stimulus  $x_n$  will be the shortest to be judged longer. Now  $P_1$  is obviously equal to  $p_1$ , and the condition that  $x_2$  may be the shortest comparison stimulus judged longer involves the probability that  $x_1$  shall not be so judged and that  $x_2$  shall. Thus we have

$$\begin{aligned} P_1 &= p_1 \\ P_2 &= (1 - p_1) p_2 \\ P_3 &= (1 - p_1) (1 - p_2) p_3 \\ &\text{-----} \\ P_7 &= (1 - p_1) (1 - p_2) \text{-----} (1 - p_6) p_7 \end{aligned}$$

If  $P_0$  represent the probability that no comparison stimulus of the series  $x_1, x_2, \text{-----} x_7$  be judged longer, we have

$$P_0 = (1 - p_1) (1 - p_2) \text{-----} (1 - p_7).$$

Since we are dealing with mutually exclusive phenomena, in the fact that  $P_1 + P_2 + P_3 + \text{-----} + P_7 + P_0 = 1$  we have a convenient check in these calculations. Having found  $P_1, P_2$ , etc., and forming the products  $x_1 P_1, x_2 P_2$ , etc., we have in  $\Sigma x_k$  the mathematical expectation of the shortest stimulus to be judged longer,—that is, a theoretical determination of the just perceptible positive difference.

In like manner, if  $P'_n$  represent the probability that  $x_n$  will be the longest comparison stimulus not judged longer, we begin at the other end of our series and have

$$\begin{aligned} P'_7 &= (1 - p_7) \\ P'_6 &= p_7 (1 - p_6) \\ P'_5 &= p_7 p_6 (1 - p_5) \\ &\text{-----} \\ P'_1 &= p_7 p_6 \text{-----} p_2 (1 - p_1) \end{aligned}$$

Here again we have a check on our calculations in that  $P'_7 + P'_6 + \text{-----} + P'_0 = 1$  where  $P'_0 = p_7 p_6 \text{-----} p_1$  and is the probability that all the comparison stimuli will be judged longer. Having found  $P'_7$ , etc., we form the products

$x_7 P'_7$ ,  $x_6 P'_6$ , etc., and have for the just perceptible positive difference the value  $\Sigma_{x_k} P'_k$ .

We may compute the negative differences in a similar manner. If  $q_1, q_2, q_7, \dots, q_7$  are the relative frequencies or probabilities of the comparison stimuli  $x_1, x_2, \dots, x_7$  being judged shorter, and if  $Q_n$  represent the probability that a comparison stimulus  $x_n$  will be the longest on which the judgment "shorter" is given, we have

$$\begin{aligned} Q_7 &= q_7 \\ Q_6 &= (1 - q_7) q_6 \\ Q_5 &= (1 - q_7) (1 - q_6) q_5 \\ &\dots \dots \dots \\ Q_1 &= (1 - q_7) (1 - q_6) \dots \dots (1 - q_2) q_1 \end{aligned}$$

Then  $Q_0$  (the probability that no comparison line will be judged shorter)  $= (1 - q_7) (1 - q_6) \dots \dots (1 - q_1)$ . As before,  $Q_7 + Q_6 + \dots + Q_0 = 1$ . Forming the products  $x_7 Q_7, x_6 Q_6$ , etc., we have in  $\Sigma_{x_k} Q_k$  the mathematical expectation of the longest comparison stimulus to be judged shorter,—that is, the theoretical value of the just perceptible negative difference. Also, if  $Q'_n$  represent the probability that a comparison stimulus  $x_n$  will be the shortest on which the judgment "shorter" is not given, we have

$$\begin{aligned} Q'_1 &= (1 - q_1) \\ Q'_2 &= q_1 (1 - q_2) \\ Q'_3 &= q_1 q_2 (1 - q_3) \\ &\dots \dots \dots \\ Q'_7 &= q_1 q_2 \dots \dots q_6 (1 - q_7) \\ \text{and } Q'_0 &= q_1 q_2 \dots \dots q_6 q_7. \end{aligned}$$

That  $Q'_1 + Q'_2 + \dots + Q'_7 + Q'_0 = 1$  serves as a check as in the previous calculations and in  $\Sigma_{x_k} Q'_k$  we have a theoretical value of the just imperceptible negative difference.

By taking the difference between the mean of the just percep-

tible and just imperceptible positive differences and the mean of the just perceptible and just imperceptible negative differences we get a theoretical value of the interval of uncertainty, or difference threshold. This is equal in the case of each subject to

$$\frac{\sum x_k P_k + \sum x_k P'_k}{2} - \frac{\sum x_k Q_k + \sum x_k Q'_k}{2}$$

These calculated values of the thresholds and of the intervals of uncertainty for each of the twelve subjects are placed in Table VIII (p. 56) in parallel columns with the observed or empirical values by the method of just perceptible differences and with the values obtained by interpolation. It is interesting to compare the results.

If we arrange our twelve subjects according to the magnitude of the interval of uncertainty as obtained by interpolation and in a parallel column we arrange them according to the observed value of the interval by the method of just perceptible differences, we have the following result.

Comparison of intervals of uncertainty (1) by interpolation and (2) by the method of just perceptible differences.

(1)		(2)	
VI.....	8.52	VI.....	4.15
VII.....	5.41	XII.....	3.94
XI.....	3.96	XI.....	3.67
III.....	3.09	VII.....	3.53
XII.....	3.07	II.....	2.82
II.....	2.93	IX.....	2.76
V.....	2.57	I.....	2.72
IX.....	2.47	V.....	1.71
I.....	2.24	III.....	1.55
IV.....	1.96	IV.....	1.23
X.....	0.92	X.....	1.05
VIII.....	0.36	VIII.....	0.51

Although the values obtained by the two methods are in some cases quite different, there is a general similarity in the two lists and the relative positions of the different subjects are much the same. If we leave subjects III and V out of account, the order of the subjects in the two lists is the same except that subjects XII and VII change places.

If we consider the middle points of these observed intervals of uncertainty, *i.e.*, the mean of the thresholds in the directions of increase and decrease respectively, as we did above (p. 23) in the case of the values by interpolation, we find a similar result. The mean for the twelve subjects is 150.05, ranging from 151.71 (Subject III) to 148.57 (Subject IX). In this case also there are six above and six below the mean value. This may confirm us in our conclusion that there was no uniform tendency to over- or under-estimation.

A comparison of the theoretical intervals of uncertainty calculated by the method of just perceptible differences with the observed or empirical values by the same method is less gratifying. As appears in the following comparative table, Subjects VI, XII, II, I, III, IV, and X maintain the same relative positions in both columns but there is little regularity in the relations of the other five.

Comparison of intervals of uncertainty by the method of just perceptible differences:

(1) calculated		(2) observed	
VI.....	6.67	VI.....	4.15
IX.....	3.44	XII.....	3.94
XII.....	3.38	XI.....	3.67
V.....	3.00	VII.....	3.53
II.....	2.78	II.....	2.82
I.....	2.35	IX.....	2.76
III.....	-2.17	I.....	2.72
XI.....	1.16	V.....	1.71
VII.....	0.69	III.....	1.55
VIII.....	0.62	IV.....	1.23
IV.....	0.57	X.....	1.05
X.....	-0.09	VIII.....	0.51

The mean of the calculated thresholds (149.00) is smaller than the observed value with practically the same mean variation among the different subjects. A study of the calculated thresholds will show that the threshold in the direction of decrease is less in every case but one (Subject VII) than the threshold by interpolation, and in every case but one (Subject XII) less than the observed threshold. Furthermore, the calculated thresh-

holds in the direction of increase are in every case smaller than the thresholds obtained by either of the other two methods for the same subjects. In a few cases<sup>3</sup> there is striking agreement in the results of the three methods.

A study of the calculations by this theoretical method of just perceptible differences shows that, in the cases of a number of subjects whose relative frequencies for shorter judgments on comparison line 144 and for longer judgments on 156 do not exceed 0.80, the probability that no one of our comparison lines will be judged shorter or longer respectively is not inconsiderable. This suggests that, for these subjects at least, our series should have been more extended. That this lack of agreement between the calculated and observed intervals of uncertainty is largely due to large values of  $P_0$  and  $Q_0$ , that is to probabilities that no comparison stimulus in our series would be judged longer or shorter respectively, and that these in turn are due to values of  $p$  and  $q$  which are less than 0.80 is borne out by the following comparison. We have arranged our subjects in order according to the extent to which their calculated intervals of uncertainty vary from the observed values and have placed in parallel columns the highest of the four values  $P_0$ ,  $P'_0$ ,  $Q_0$ , and  $Q'_0$  for each subject, and in the third and fourth columns the values of  $q_1$  and  $p_1$ .

Subject	Difference	Maximum $P_0$ , etc.	$q_1$	$p_1$
III.....	- 3.72	$P_0 = 0.064$	0.804	0.628
VII.....	- 2.84	$P_0 = 0.047$	0.782	0.680
VI.....	+ 2.52	$Q_0 = 0.068$	0.586	0.718
XI.....	- 2.51	$P_0 = 0.033$	0.900	0.796
V.....	+ 1.29	$Q_0 = 0.020$	0.752	0.842
X.....	- 1.14	$P_0 = 0.019$	0.940	0.796
IX.....	+ 0.68	$Q_0 = 0.025$	0.750	0.896
IV.....	- 0.66	$P_0 = 0.012$	0.956	0.794
XII.....	- 0.56	$Q_0 = 0.007$	0.944	0.978
I.....	- 0.37	$P_0 = 0.007$	0.878	0.862
VIII.....	+ 0.11	$Q_0 = 0.001$	0.900	0.948
II.....	- 0.04	$Q_0 = 0.003$	0.860	0.848

<sup>3</sup> Subject II had thresholds 147.28, 147.34, and 147.41 in the direction of decrease and 150.06, 150.27, and 150.23 in the direction of increase; Subject VIII had thresholds 149.53, 149.91, and 149.73 in the direction of decrease and 150.15, 150.27, and 150.24 in the direction of increase.

It may be observed from this comparison that where  $p_7$  is relatively small,  $P_0$  is large and we find a calculated interval less than the observed. On the other hand, where  $q_1$  is small,  $Q_0$  is large and, in four out of six cases, the calculated interval is larger than the observed. It seems likely from this comparison that there would have been closer agreement between the results of these two methods of deriving the difference threshold, or interval of uncertainty, if we had either (1) added comparison stimuli 142 and 158 at the lower and upper ends of our series respectively, or (2) had kept the number of our comparison pairs at seven but had used comparison lines 141, 144, 147, 150, 153, 156, and 159. Here the question also arises as to whether the same comparison pairs should be used for each subject or whether we should so adjust the comparison pairs for each subject that the relative frequency of shorter judgments on the shortest comparison line and of longest judgments on the longest line should exceed some arbitrary minimum, say 0.95. It is obvious that if we had used 141 and 159 as the extremes of our series of comparison lines in this experiment, the last five subjects in the above table would have had relative frequencies nearly equal to unity for shorter and longer judgments respectively on these lines. It may be remarked in this connection that in the preliminary experiments with a more extended series, when the present series was being decided upon, Subject II was used to a considerable extent. If Subjects III or VII had been used for this preliminary work, it is very likely that our series would have been too extended for the majority of observers for there is little point in having a comparison pair upon which the probability of a certain type of judgment amounts to a practical certainty.

### EQUALITY CASES

If we consider each equality judgment as an observation upon the value of the stimulus giving rise to the same sensation of length as our standard stimulus, we may apply the same methods

of computation as in a series of determinations of some quantity in chemical experimentation,—the atomic weight of an element, for example. We derive the arithmetical mean of all our observations as the most probable value of the magnitude to be determined. We then proceed by the method of least squares to compute the probable error of this mean result. If we have  $n$  observations varying from the mean by  $v_1, v_2$ , etc., we sum the squares of these variations and compute the probable error of the mean result by the formula

$$0.6745 \sqrt{\frac{\sum v^2}{n(n-1)}}$$

It has already been noticed that the graph of the psychometric function for equality judgments has, in typical cases, more or less of the bell shape of the error curve,—particularly if we consider its course in the neighborhood of the comparison stimulus equal to the standard. The maximum value of the psychometric function for equality judgments has been computed in the following manner. If the three largest values of the probability of an equality judgment for a given subject as found in our table of relative frequencies are  $p_a, p_b$ , and  $p_c$ , corresponding to values  $x_a, x_b$ , and  $x_c$  of the comparison stimuli, and the value of the comparison stimulus corresponding to the maximum of the psychometric function for equality judgments  $x_m$  differ from our middle value  $x_b$  by  $d$ , we have, on the assumption of parabolic form,

$$d = \frac{p_a - p_c}{2(p_a + p_c - 2p_b)}, \text{ and} \\ x_m = x_b + d.$$

It is of course necessary to observe the sign of  $d$  as it is sometimes positive and sometimes negative. We may then find the maximum value of the psychometric function for equality judgments  $p_m$  by introducing  $x_m$  into Lagrange's formula.

It is interesting in connection with the use of error methods in psychophysics to compare the values of the maximum of the psychometric function for equality judgments with the values of

the probable error of the mean result for our different subjects. The result is what might be expected from the similarity of the course of the equality function and the error curve. It will be noticed that as the maximum of the equality function decreases (column 1), the probable error increases. This comparison is rather striking when we recall the earlier observations made upon the diversities of the number and distribution of these very equality judgments.

Subject	Maximum	Probable Error
XII.....	0.536	0.058
XI.....	0.443	0.070
VII.....	0.396	0.077
VI.....	0.394	0.078
I.....	0.323	0.087
III.....	0.285	0.092
II.....	0.220	0.100
IX.....	0.212	0.105
V.....	0.204	0.110
IV.....	0.192	0.117
X.....	0.111	0.150
VIII.....	0.069	0.175

It is to be expected from the fact that the sum of the three ordinates for the psychometric functions for any comparison stimulus is equal to one that this maximum of the equality function is to be found where the values for the shorter and longer judgments are both small,—that is, in the neighborhood of the interval of uncertainty. It will be seen by reference to the tabulation of the analysis of equality judgments (p. 55) that this value of the comparison stimulus  $x_m$  lies within the interval of uncertainty both as observed by the method of just perceptible differences and as determined by interpolation for eight of our twelve subjects (the exceptions being V, VII, VIII, and X). We note further that  $x_m$  has a mean value of 149.91 with considerable variation (M.V. = 1.68).

If, as above, we consider each of these equality judgments as determinations of a certain value  $x$ , we find that the mean of the equality judgments of each subject differs but little from 150 with an average of 149.94 for all twelve subjects and a mean

variation of 0.69. If we make allowance for the different number of this class of judgments made by each the result is much the same. We have in all 7445 equality judgments with a mean value of 149.89. These results agree with our study of shorter and longer judgments in showing that there is no marked tendency to either under- or over-estimate the comparison stimulus.

If we consider the absolute frequency of the equality judgments made upon each of our seven comparison stimuli as weights of these seven observations upon our standard stimulus we may compute the probable error of a single result from the formula

$$0.6745 \sqrt{\frac{\sum v^2}{6}}$$

wher  $\sum v^2$  is the sum of the weighted squares of the variations from the mean result. These results are tabulated in column 5 of the analysis of equality judgments on page 55. It is of interest to compare them with some other test of the sensitivity of our subjects. If these are arranged according to the length of the interval of uncertainty by interpolation and according to the probable errors just derived, we have the following:

Interval of uncertainty by interpolation		Probable error of a single result	
VI.....	8.52	VI.....	32.43
VII.....	5.41	VII.....	31.16
XI.....	3.96	XI.....	26.31
III.....	3.09	III.....	25.78
XII.....	3.07	I.....	23.33
II.....	2.93	V.....	22.99
V.....	2.57	IX.....	22.34
IX.....	2.47	II.....	22.14
I.....	2.24	XII.....	20.19
IV.....	1.96	IV.....	16.08
X.....	0.92	X.....	15.91
VIII.....	0.36	VIII.....	9.62

It will be noticed that, except for Subjects I, III, and XII, these orders are the same.

The following table makes a more general comparison possi-

ble. In the first column the subjects are arranged according to the length of the interval of uncertainty by interpolation, in the second according to the length of the interval by the method of just perceptible differences, in the third according to the probable error of the mean of the equality judgments, in the fourth according to the maximum of the equality function, and in the fifth according to the probable error of a single equality determination. (Subjects III and XII are omitted.)

{	(1) VI	(2) VI	(3) XI	(4) XI	(5) VI	}	A
	VII	XI	VII	VII	VII		
	XI	VII	VI	VI	XI		
{	II	II	I	I	I	}	B
	V	IX	II	II	V		
	IX	I	IX	IX	IX		
{	I	V	V	V	II	}	C
	IV	IV	IV	IV	IV		
	X	X	X	X	X		
	VIII	VIII	VIII	VIII	VIII		

This falls short of the remarkable agreement of Dr. Urban's four arrangements of his seven subjects but is confirmatory in a general way of his main conclusion "that there must exist some kind of relation between the values of the different psychometric functions, and there must also exist a relation between the probable error and the maximum value of the psychometric function of the equality judgments". It may be noticed that, if we divide the ten subjects listed above into three groups A, B, and C, putting the first three in A, the next four in B, and the last three in C, no subject from one group in any arrangement appears out of his group in any other.

It is obvious that as one increases the number of his subjects, there is less likelihood that he will find perfect agreement in different serial arrangements. He will look rather for slight variations in any one subject from an average position. The expectation of absolute uniformity in serial arrangement for a number of subjects will be less in the middle of a series than at its extremes for, if the number of subjects is increased, it is not likely that the range of values for any of these tests of

sensitivity will be greatly changed and so we may expect more additions to find place in the middle of any column than at its extremes. As these middle values approximate a mean, it requires only slight differences in them to effect a change in the serial order. This is shown in our present experiment by the fact that, if we include all twelve subjects in an arrangement like the above, in column (1) six values will lie within a range of 8.4 per cent of the difference between the first and last in the column, in column (2) five within a range of 30.5 per cent, in column (3) six within a range of 24.5 per cent, in column (4) five within a range of 25.5 per cent, and in column (5) five within a range of 19.2 per cent.

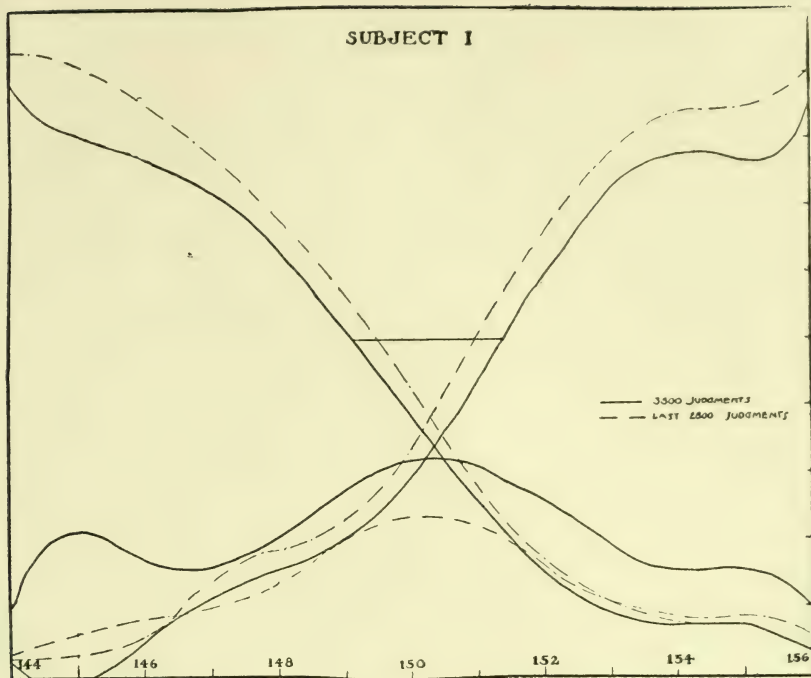
## DESCRIPTION OF APPARATUS

As stated above (p. 2), the apparatus used was particularly designed to serve in this experiment. It is essentially a revolving drum (A in Fig. 2, p. 49) mounted on an axle behind a cardboard screen (D) and driven by an "intermittent gear" (B) which in turn was driven by an electric motor. The drum is a fourteen-sided prism made of light wood. On seven alternate faces are nailed thin strips of wood,  $\frac{3}{8}'' \times 3\frac{1}{2}'' \times 12''$ . The intermediate faces are covered with white paper. Each of the seven raised faces is fitted with six small brass fasteners projecting  $\frac{1}{4}''$  over the edges of the outer surface. These fasteners hold the cards, also  $3\frac{1}{2}'' \times 12''$ , firmly in place and yet allow the cards to be changed easily. On the end of the drum nearer the recorder these seven faces are distinguished by numbers from 1 to 7. The axles for the drum and the intermittent gear wheels are  $\frac{1}{2}''$  in diameter and rest in bearings mounted on a box framework of  $\frac{7}{8}''$  white pine. The drum axle is held tightly enough by its bearings so that it will not turn very freely but retains the position in which the intermittent gear leaves it. The wheel O is made fast to the drum axle. It is made with seven rests which allow the drive wheel to continue its revolution and yet leave the drum at rest. Belts such as are used for

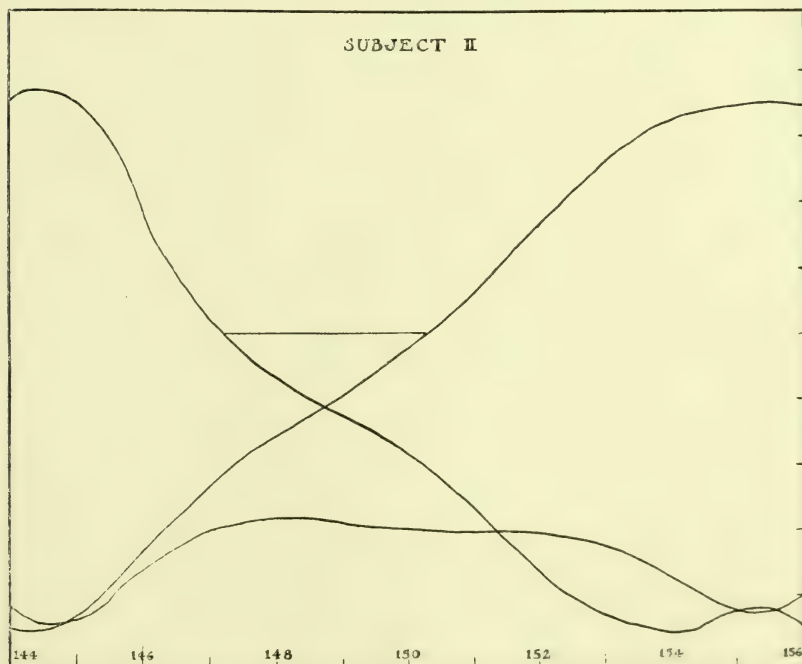
sewing-machines connect the apparatus to a speed-reducer (S) which is connected to a small motor. The belts which revolve most rapidly are sewed together rather than held by the ordinary metal fasteners in order to prevent any clicking sounds as they pass over the wheels.

Drum, screen, speed-reducer, and motor were all set up on a large laboratory table (T) along one side of the experiment room. The subject sat behind a small table facing the screen; the recorder sat where he could see the numbers on the drum. The screen consists of a frame holding a sheet of white cardboard into which a horizontal opening (C)  $2\frac{1}{2}'' \times 10''$  is cut at such a height from the table that when any card (as No. 3 in Fig. 2) is brought into position, the center of the card is behind the center of the opening. This screen is high enough to shut off entirely from the subject any view of the rest of the apparatus. The margins of the cards do not appear when at rest and, except for a slight shadow, there is little break in the continuity of the background on which the comparison lines are seen.

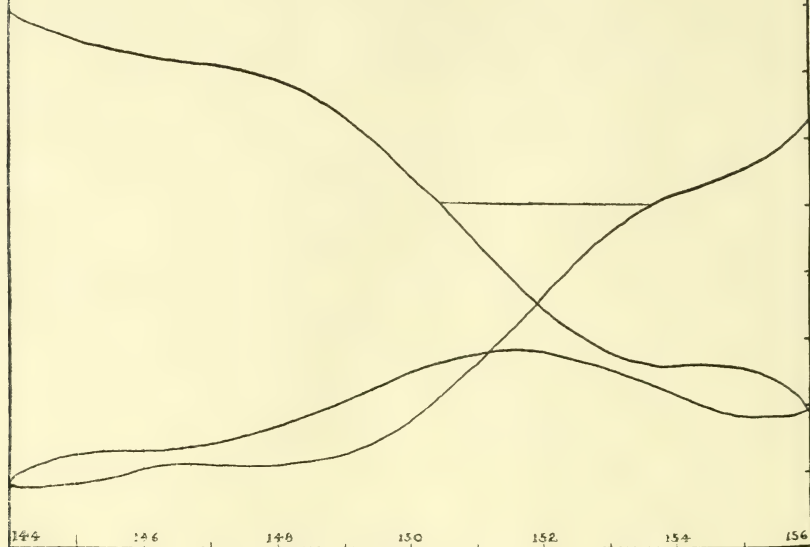
# SUBJECT I



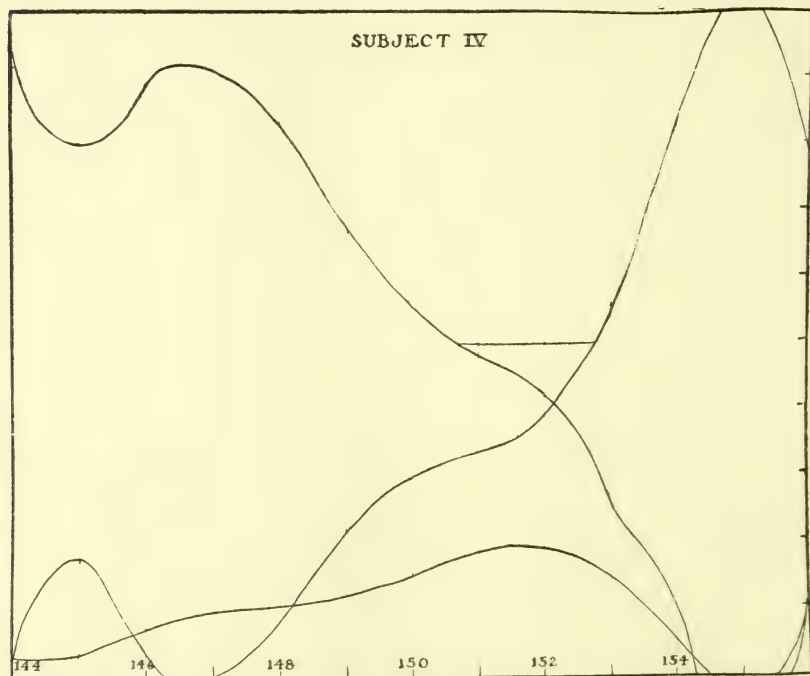
# SUBJECT II



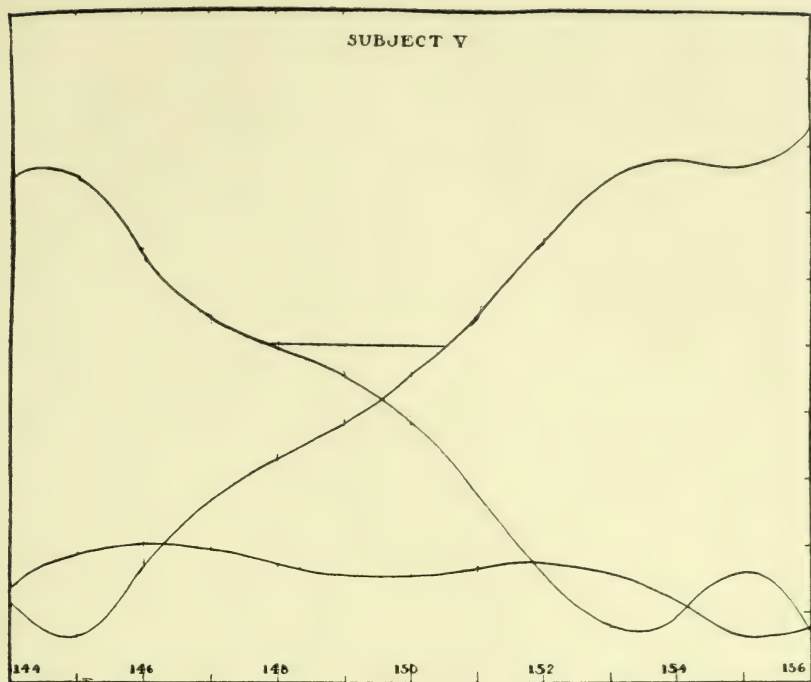
SUBJECT III



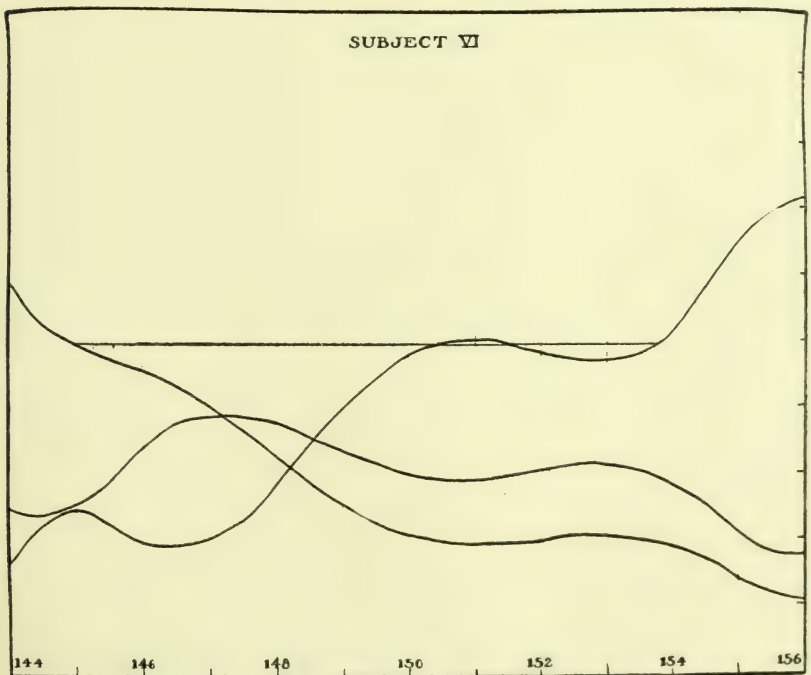
SUBJECT IV

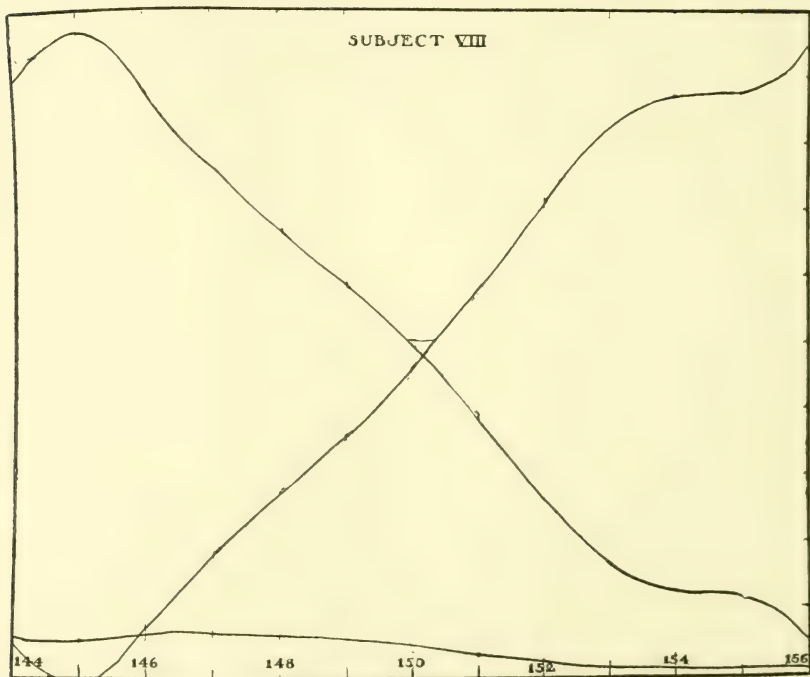
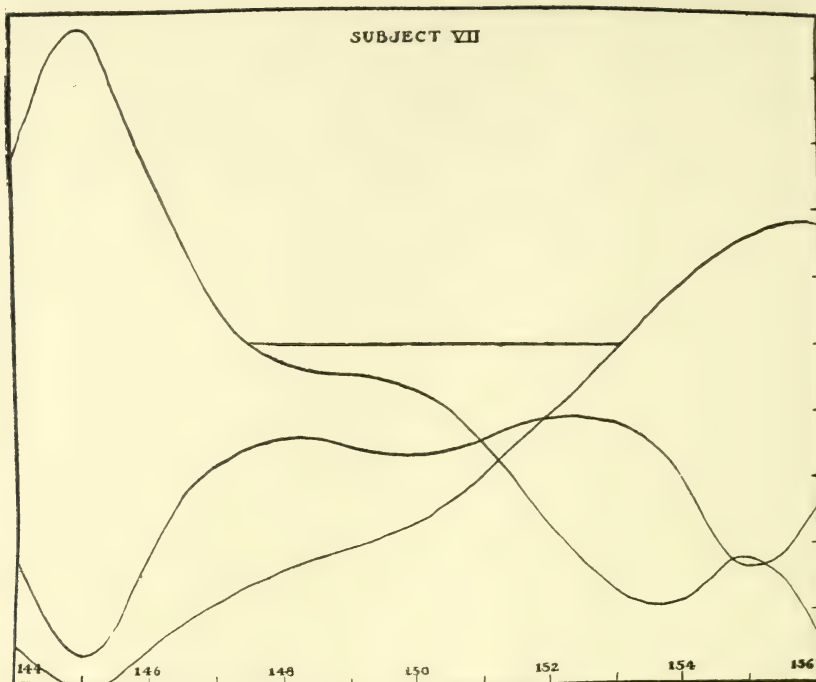


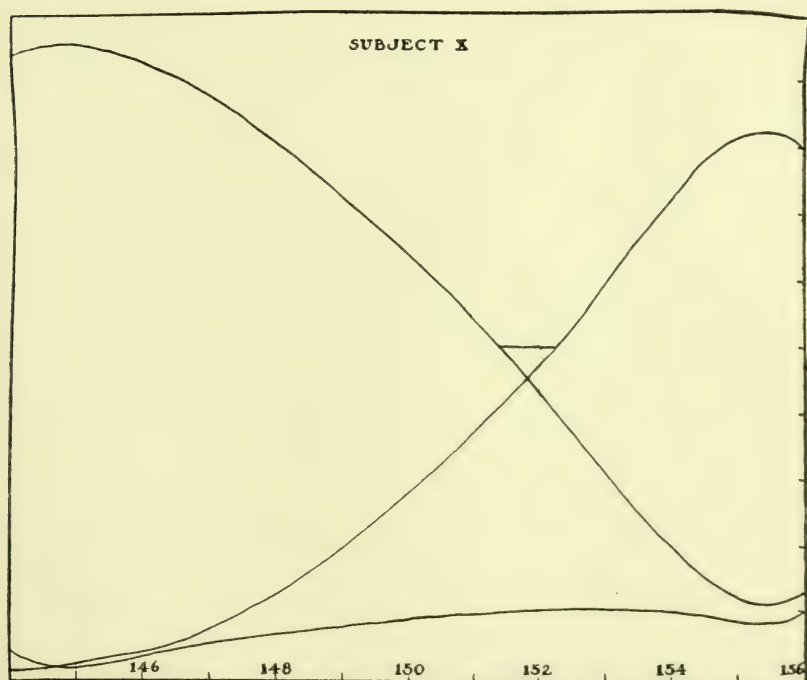
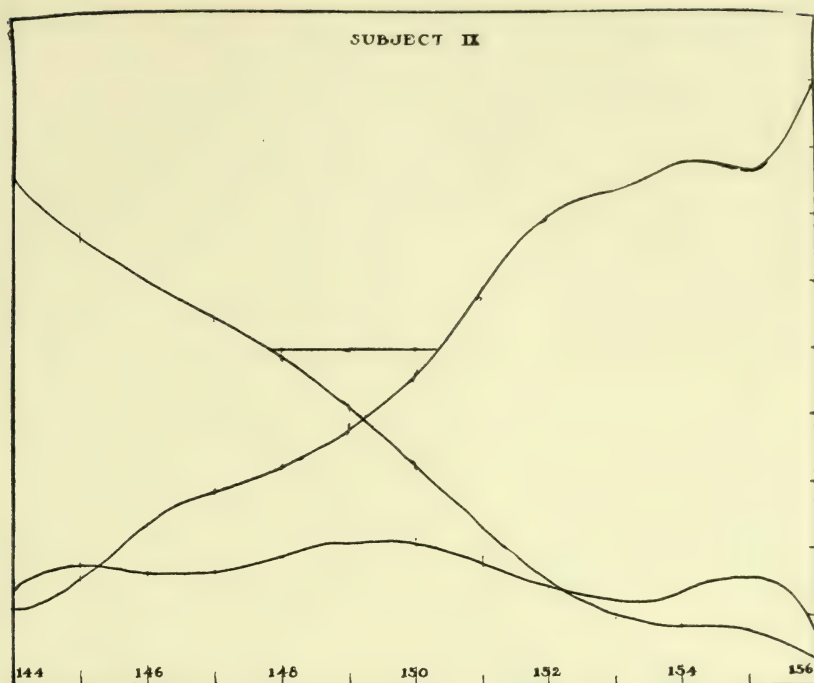
SUBJECT V



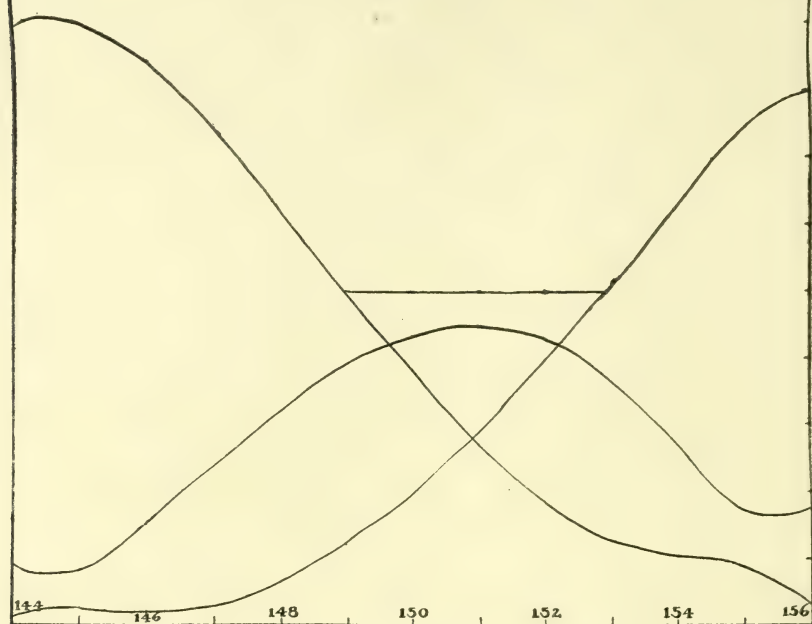
SUBJECT VI



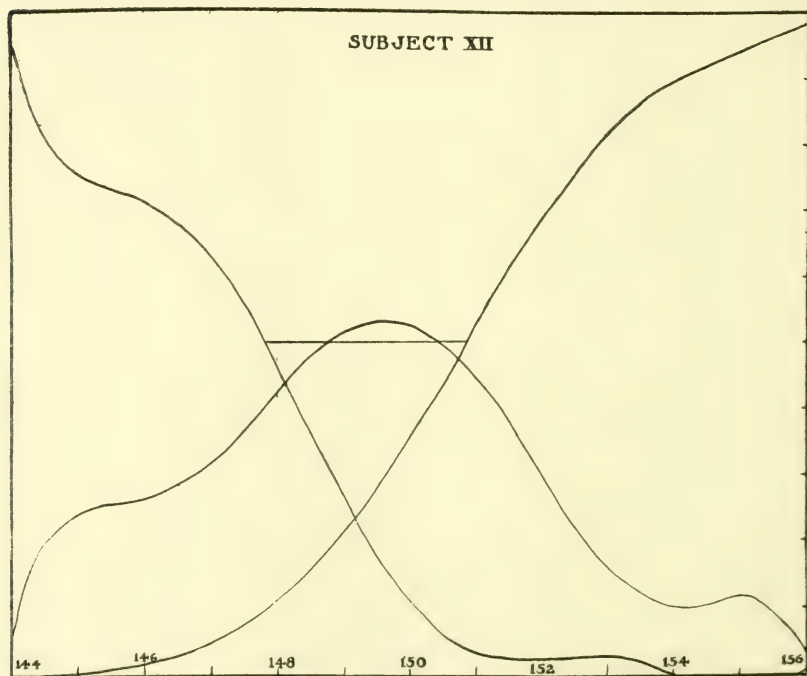




# SUBJECT XI



# SUBJECT XII



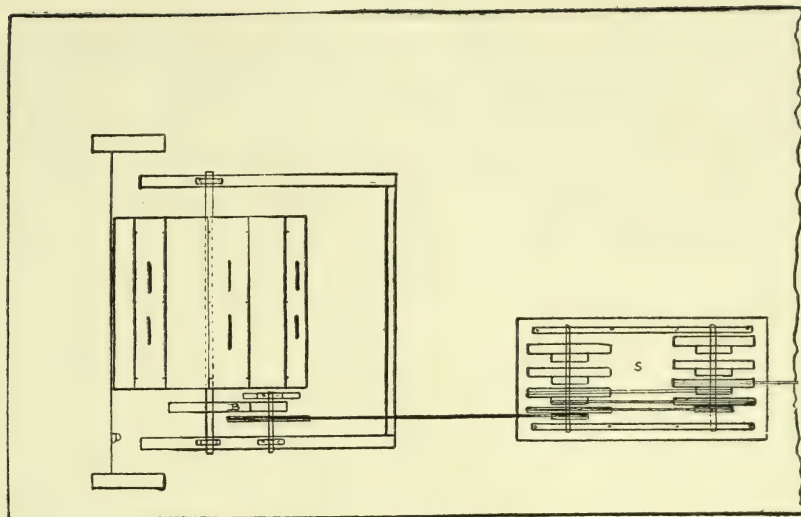


Figure 1

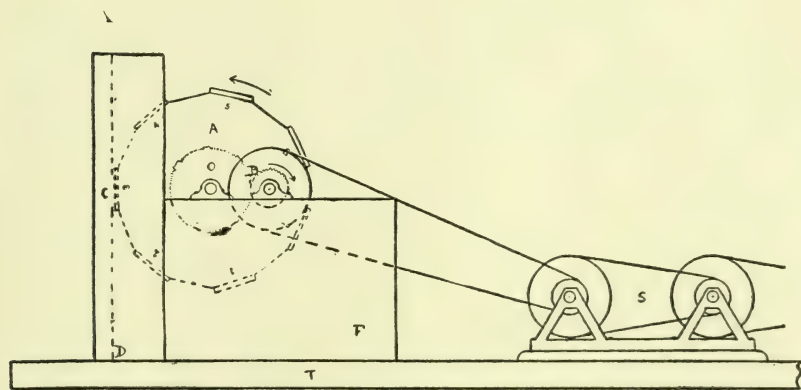


Figure 2

TABLE I. ABSOLUTE FREQUENCIES OF JUDGMENTS

Subject I

C. S.	144			146			148			150			152			154			156		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	31	19	0	20	30	0	20	28	2	15	30	5	3	31	16	6	22	22	2	15	33
b	34	16	0	23	26	1	26	22	2	7	36	7	5	30	15	2	20	28	1	13	36
IIa	46	3	1	43	6	1	32	11	7	20	13	17	5	20	25	4	13	33	2	5	43
b	42	7	1	41	4	5	28	15	7	12	20	18	8	17	25	2	9	39	6	6	38
IIIa	44	3	3	39	7	4	24	10	16	18	13	19	7	5	38	2	1	47	3	2	45
b	47	0	3	40	8	2	42	3	5	22	18	10	1	5	44	2	1	47	0	1	49
IVa	47	1	2	44	0	6	41	1	8	28	5	17	16	2	32	9	1	40	3	1	46
b	49	0	1	47	3	0	40	5	5	13	19	18	14	16	20	4	8	38	0	4	46
Va	49	1	0	46	4	0	38	6	6	30	6	14	9	1	40	2	2	46	2	3	45
b	50	0	0	44	1	5	29	2	19	29	0	21	8	0	42	3	0	47	0	0	50
Σ	439	50	11	387	89	24	320	103	77	194	160	146	76	127	297	36	77	387	19	50	431

Subject II

C. S.	144			146			148			150			152			152			154		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	46	3	1	39	8	3	26	18	6	25	21	4	12	16	22	3	16	31	3	15	32
b	46	2	2	39	10	1	32	8	10	23	15	12	5	37	8	5	7	38	4	10	36
IIa	37	12	1	38	8	4	18	19	13	10	16	24	6	6	38	4	9	37	6	10	34
b	37	11	2	37	10	3	21	17	12	15	13	22	8	13	29	3	10	37	4	13	33
IIIa	42	2	6	20	5	25	22	2	6	13	6	31	6	3	41	1	1	48	0	3	47
b	44	2	4	30	2	18	13	6	31	16	3	31	5	2	43	2	3	45	2	1	47
IVa	45	1	4	37	7	6	25	7	18	9	7	34	8	8	34	0	6	44	0	0	50
b	45	3	2	34	7	9	15	17	18	21	4	25	7	7	36	0	6	44	3	1	46
Va	42	5	3	33	5	12	23	11	16	16	13	21	7	3	40	5	3	42	0	1	49
b	46	2	2	37	9	4	24	5	21	12	4	34	5	3	42	0	2	48	0	0	50
Σ	430	43	27	344	71	85	219	110	171	160	102	238	69	98	333	23	63	414	22	54	424

Subject III

C. S.	144			146			148			150			152			154			156		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	13	20	17	14	16	20	14	19	17	19	15	16	7	21	22	21	13	16	9	21	20
b	21	17	12	18	18	14	10	23	17	23	12	15	13	16	21	11	14	25	14	18	18
IIa	34	4	12	34	4	12	36	4	10	19	7	24	16	3	31	12	0	38	16	4	30
b	45	2	3	46	0	4	46	3	1	31	10	9	24	2	24	9	5	36	19	2	29
IIIa	47	0	3	39	5	6	38	6	6	37	7	6	25	5	20	14	6	30	14	5	31
b	50	0	0	40	7	3	40	7	3	35	13	2	14	18	18	15	9	26	3	9	38
IVa	46	4	0	43	7	0	40	6	4	22	21	7	18	21	11	10	19	21	5	10	35
b	50	0	0	46	4	0	44	4	2	29	16	5	23	19	8	10	18	22	4	10	36
Va	47	3	0	46	4	0	42	8	0	31	14	5	16	17	17	12	9	29	2	7	41
b	49	1	0	42	8	0	38	10	2	33	13	4	19	19	12	16	16	18	4	10	36
Σ	402	51	47	368	73	59	348	90	62	279	128	93	175	141	184	130	109	261	90	96	314

TABLE II. ABSOLUTE FREQUENCIES OF JUDGMENTS

Subject IV

C. S.	144			146			148			150			152			154			156		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	41	4	5	36	12	2	28	18	4	26	20	4	20	26	4	10	12	28	8	26	16
b	48	2	0	34	12	4	32	16	2	23	24	3	26	13	11	6	9	35	10	15	25
Ila	50	0	0	47	2	1	39	5	6	30	7	7	13	17	20	8	2	40	2	1	47
b	48	0	2	45	1	4	45	1	4	38	4	8	17	9	24	8	0	42	2	0	48
IIla	46	4	0	44	5	1	49	1	0	39	3	8	24	11	15	7	1	42	5	1	44
b	48	1	1	40	1	3	49	1	0	37	2	11	18	6	26	4	1	45	5	1	44
IVa	49	1	0	49	1	0	37	8	5	38	7	5	31	11	8	4	0	46	3	3	44
b	49	0	1	47	0	3	41	1	8	27	7	16	30	2	18	0	0	50	1	0	49
Va	49	0	1	47	0	3	47	0	3	4	0	46	11	0	39	1	0	49	8	0	42
b	50	0	0	49	1	0	45	0	5	10	0	40	24	0	26	2	0	48	10	2	38
Σ	478	11	11	444	34	22	412	51	37	278	74	148	214	95	191	50	25	425	54	49	397

Subject V

C. S.	144			146			148			150			152			154			156		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	35	9	6	25	17	8	25	18	7	19	20	11	6	25	19	4	15	31	5	11	34
b	35	13	2	29	16	5	31	12	7	26	15	9	14	16	20	3	9	38	2	14	34
Ila	38	9	3	41	7	2	30	6	14	27	6	17	17	10	23	9	8	33	7	3	40
b	43	7	0	37	10	3	28	8	14	20	8	22	14	14	22	4	13	33	6	5	39
IIla	41	6	3	28	8	14	29	8	13	29	9	12	5	7	38	6	6	38	5	2	43
b	40	2	8	28	4	18	26	3	21	23	1	26	5	2	43	6	1	43	2	0	48
IVa	28	9	13	27	21	2	12	16	22	10	5	35	0	6	44	1	0	49	1	1	48
b	33	11	6	23	19	8	11	12	27	10	12	28	5	8	37	2	7	41	1	3	46
Va	40	0	10	36	0	14	27	0	23	14	0	36	7	0	43	8	0	42	5	0	45
b	43	2	5	41	0	9	29	4	17	17	3	30	7	0	43	6	1	43	5	1	44
Σ	376	68	56	315	102	83	248	87	165	195	79	226	80	88	332	49	60	391	39	40	421

Subject VI

C. S.	144			146			148			150			152			154			156		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	24	25	1	27	18	5	15	25	10	16	25	9	9	24	17	8	20	22	7	11	32
b	34	14	2	21	27	2	18	29	3	10	27	13	8	24	18	3	13	34	3	19	28
Ila	19	26	5	23	21	6	15	33	2	8	11	31	12	21	17	1	23	26	0	10	40
b	23	17	10	20	21	9	12	21	17	9	12	29	12	10	28	4	5	41	3	4	43
IIla	18	14	18	17	16	17	7	12	31	6	17	27	28	8	14	29	12	9	15	12	23
b	28	9	13	21	7	22	12	15	23	11	16	23	7	10	33	9	12	29	9	13	28
IVa	26	11	13	34	15	1	11	14	25	8	7	35	16	17	17	9	18	23	1	2	47
b	42	2	6	20	16	14	22	18	10	6	8	36	5	11	34	11	15	24	8	10	32
Va	32	4	14	23	16	11	30	10	10	13	13	24	1	15	34	13	11	26	7	6	37
b	47	2	1	24	15	11	24	13	13	19	15	16	4	15	31	8	13	29	0	1	49
Σ	293	124	83	230	172	98	166	190	144	106	151	243	102	155	243	95	142	263	53	88	359

TABLE III. ABSOLUTE FREQUENCIES OF JUDGMENTS

Subject VII

C. S.	144			146			148			150			152			154			156		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	39	11	0	34	14	2	14	33	3	19	25	6	11	24	15	3	21	26	2	9	39
Ib	35	15	0	32	17	1	16	29	5	18	24	8	4	18	28	4	16	30	0	16	34
IIa	40	6	4	43	6	1	29	10	11	26	12	12	9	22	19	5	11	34	3	11	36
Ib	40	8	2	37	10	3	30	13	7	23	21	6	6	22	22	3	16	31	5	17	28
IIIa	41	5	4	39	7	4	32	7	11	32	6	12	18	20	12	7	13	30	3	16	31
Ib	41	5	4	36	10	4	25	15	10	26	11	13	19	17	14	9	11	30	4	13	33
IVa	40	9	1	41	7	2	18	21	11	10	19	21	19	21	10	5	16	29	2	10	38
Ib	35	12	3	40	6	4	21	16	13	15	18	17	14	17	19	8	19	23	5	12	33
Va	42	6	2	40	7	3	34	11	5	24	16	10	8	17	25	6	17	27	3	12	35
Ib	38	8	4	39	11	0	17	26	7	24	16	10	6	18	26	8	5	37	4	13	33
Σ	391	85	24	381	95	24	236	181	83	217	168	115	114	196	190	58	145	297	31	129	340

Subject VIII

C. S.	144			146			148			150			152			154			156		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	47	2	1	41	7	2	31	6	13	29	3	18	12	1	37	7	1	42	0	1	49
Ib	40	10	0	31	14	5	18	14	18	31	7	12	7	1	42	1	2	47	0	2	48
IIa	49	0	1	46	0	4	37	0	13	28	0	22	21	0	29	10	0	40	1	0	49
Ib	50	0	0	50	0	0	42	0	8	27	0	23	12	0	38	7	0	43	2	0	48
IIIa	42	1	7	36	1	13	30	0	18	29	2	19	19	2	29	11	0	39	4	1	45
Ib	49	0	1	46	0	4	44	0	6	36	1	13	19	0	31	14	0	36	8	0	42
IVa	36	10	4	41	7	2	21	11	18	10	6	34	12	5	33	0	1	49	1	0	49
Ib	43	5	2	45	2	3	28	3	19	20	3	27	22	1	27	2	0	48	2	0	48
Va	46	0	4	49	0	1	36	0	14	17	0	33	3	0	47	4	0	46	4	0	46
Ib	48	0	2	47	0	3	40	0	10	19	2	29	3	0	47	5	0	45	0	0	50
Σ	450	28	22	432	31	37	329	34	137	246	24	230	130	10	360	61	4	435	22	4	474

Subject IX

C. S.	144			146			148			150			152			154			156		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	32	12	6	32	9	9	17	21	12	11	20	19	12	12	26	0	14	36	1	5	44
Ib	33	9	8	31	11	8	20	14	16	15	18	17	5	12	33	2	16	32	2	3	45
IIa	34	10	6	36	5	9	12	17	21	13	13	24	12	7	31	9	7	34	3	3	44
Ib	35	4	11	28	10	12	20	12	18	14	10	26	8	10	32	6	3	41	2	4	44
IIIa	37	9	4	28	3	19	20	8	22	19	6	25	4	3	43	5	3	42	1	0	49
Ib	36	11	3	18	18	14	21	7	22	12	11	27	8	4	38	5	1	44	1	6	43
IVa	45	4	1	31	7	12	38	3	9	19	7	24	3	5	42	5	12	33	1	2	47
Ib	43	4	3	29	5	16	26	8	16	21	7	22	11	6	33	4	6	40	0	2	48
Va	41	2	7	40	4	6	34	3	13	21	3	26	9	6	35	2	4	44	7	1	42
Ib	39	5	6	30	9	11	37	1	12	19	9	22	7	6	37	4	2	44	1	7	42
Σ	375	70	55	303	81	116	245	94	161	164	104	232	79	71	350	42	68	390	19	33	448

TABLE IV. ABSOLUTE FREQUENCIES OF JUDGMENTS

Subject X

C. S.	144			146			148			150			152			154			156		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	38	11	1	40	8	2	32	17	1	20	21	9	10	25	15	6	24	20	7	26	17
Ib	43	4	3	43	3	4	38	2	10	24	12	14	25	6	19	9	11	30	11	5	34
Ila	49	1	0	50	0	0	39	2	9	35	1	14	23	3	24	10	1	39	8	1	41
Ilb	49	0	1	46	2	2	40	0	10	32	0	18	23	1	26	8	0	42	2	1	47
IIla	50	0	0	45	0	5	41	1	8	28	1	21	26	2	22	9	0	41	4	0	46
Ilb	50	0	0	45	1	4	47	0	3	35	0	15	23	0	27	10	0	40	2	0	48
IVa	50	0	0	50	0	0	43	0	7	31	1	18	29	0	21	9	1	40	5	1	44
Ib	48	0	2	49	0	1	38	0	12	33	0	17	21	0	29	5	0	45	10	0	40
Va	49	0	1	48	0	2	43	4	3	40	1	9	15	2	33	14	0	36	3	0	47
Ib	44	6	0	48	2	0	43	7	0	42	6	2	18	14	18	15	14	21	8	8	34
Σ	470	22	8	464	16	20	404	33	63	320	43	137	213	53	234	95	51	354	60	42	398

Subject XI

C. S.	144			146			148			150			152			154			156		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	43	6	1	42	8	0	36	9	5	15	25	10	9	20	21	5	9	36	1	11	38
Ib	40	10	0	35	13	2	25	23	2	16	18	16	11	23	16	5	7	38	4	6	40
Ila	43	7	0	46	4	0	27	22	1	22	21	7	9	23	18	3	15	32	1	7	42
Ilb	49	1	0	41	9	0	35	13	2	23	18	9	7	23	20	4	13	33	0	7	43
IIla	50	0	0	42	8	0	26	23	1	22	24	4	10	18	22	2	12	36	0	6	44
Ib	36	10	4	39	6	5	26	14	10	18	20	12	12	19	19	11	8	31	5	12	33
IVa	47	2	1	45	5	0	32	17	1	14	23	13	9	30	11	4	22	24	2	7	41
Ib	48	2	0	46	4	0	28	15	7	17	25	8	7	24	19	3	12	35	2	13	35
Va	46	4	0	41	7	2	36	11	3	25	20	5	10	18	22	6	16	28	0	9	41
Ib	48	2	0	37	13	0	38	12	0	16	19	15	8	14	28	9	18	23	0	9	41
Σ	450	44	6	414	77	9	309	159	32	188	213	99	92	212	196	52	132	316	15	87	398

Subject XII

C. S.	144			146			148			150			152			154			156		
Series	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l	s	e	l
Ia	48	2	0	43	7	0	43	6	1	6	36	8	2	32	16	1	11	38	0	5	45
Ib	49	1	0	40	10	0	40	9	1	3	26	21	2	28	20	0	6	44	0	1	49
Ila	47	3	0	40	10	0	20	25	5	7	26	17	2	10	38	0	4	46	0	0	50
Ilb	44	5	1	42	8	0	16	29	5	5	27	18	2	9	39	0	3	47	0	1	49
IIla	47	3	0	33	15	2	18	23	9	7	23	20	0	10	40	0	2	48	0	0	50
Ib	49	1	0	31	16	3	14	23	13	4	17	29	0	8	42	0	1	49	0	0	50
IVa	49	1	0	37	13	0	23	23	4	2	32	16	1	19	30	1	7	42	0	1	49
Ib	48	2	0	25	25	0	20	29	1	8	35	7	1	15	34	0	8	42	0	3	47
Va	47	3	0	33	17	0	16	23	11	7	24	19	0	8	42	0	2	48	0	0	50
Ib	44	6	0	35	12	3	22	22	6	6	19	25	0	9	41	0	6	44	0	0	50
Σ	472	27	1	359	133	8	232	212	56	55	265	180	10	148	342	2	50	448	0	11	489

TABLE V. RELATIVE FREQUENCIES OF JUDGMENTS.

I				II			III		
	s	e	l	s	e	l	s	e	l
144	0.878	0.100	0.022	0.860	0.086	0.054	0.804	0.102	0.094
145	0.810	0.218	[—] .029	0.860	0.070	0.071	0.762	0.141	0.097
146	0.774	0.178	0.048	0.688	0.142	0.170	0.736	0.146	0.118
147	0.724	0.162	0.114	0.533	0.200	0.267	0.721	0.155	0.125
148	0.640	0.206	0.154	0.438	0.220	0.342	0.696	0.180	0.124
149	0.523	0.275	0.201	0.380	0.214	0.406	0.643	0.218	0.139
150	0.388	0.320	0.292	0.320	0.204	0.476	0.558	0.256	0.186
151	0.257	0.311	0.433	0.236	0.200	0.564	0.452	0.281	0.268
152	0.152	0.254	0.594	0.138	0.196	0.666	0.350	0.282	0.368
153	0.090	0.187	0.722	0.063	0.175	0.762	0.282	0.258	0.461
154	0.072	0.154	0.774	0.046	0.126	0.828	0.260	0.218	0.522
155	0.074	0.158	0.769	0.079	0.071	0.850	0.259	0.184	0.557
156	0.038	0.100	0.862	0.044	0.108	0.848	0.180	0.192	0.628

IV				V			VI		
	s	e	l	s	e	l	s	e	l
144	0.956	0.022	0.022	0.752	0.136	0.112	0.586	0.248	0.166
145	0.796	0.029	0.175	0.751	0.184	0.065	0.500	0.255	0.246
146	0.888	0.068	0.044	0.630	0.204	0.166	0.460	0.344	0.196
147	0.915	0.091	[—] .006	0.540	0.195	0.265	0.406	0.392	0.202
148	0.824	0.102	0.074	0.496	0.174	0.330	0.332	0.380	0.288
149	0.679	0.118	0.203	0.459	0.159	0.382	0.260	0.337	0.403
150	0.556	0.148	0.296	0.390	0.158	0.452	0.212	0.302	0.486
151	0.485	0.181	0.335	0.280	0.169	0.552	0.197	0.295	0.508
152	0.428	0.190	0.382	0.160	0.176	0.664	0.204	0.310	0.486
153	0.313	0.147	0.540	0.086	0.163	0.752	0.210	0.317	0.472
154	0.100	0.050	0.850	0.098	0.120	0.782	0.190	0.284	0.526
155	[—] .105	[—] .030	1.136	0.161	0.068	0.770	0.139	0.210	0.651
156	0.108	0.098	0.794	0.078	0.080	0.842	0.106	0.176	0.718

VII				VII			IX		
	s	e	l	s	e	l	s	e	l
144	0.782	0.170	0.048	0.900	0.056	0.044	0.750	0.140	0.110
145	0.973	0.039	[—] .012	0.966	0.055	[—] .020	0.668	0.177	0.155
146	0.762	0.190	0.048	0.864	0.062	0.074	0.606	0.162	0.232
147	0.559	0.322	0.119	0.748	0.068	0.184	0.551	0.165	0.284
148	0.472	0.362	0.166	0.658	0.068	0.274	0.490	0.188	0.322
149	0.457	0.347	0.196	0.581	0.060	0.358	0.415	0.209	0.375
150	0.434	0.336	0.230	0.492	0.048	0.460	0.328	0.208	0.464
151	0.353	0.358	0.290	0.380	0.033	0.587	0.237	0.180	0.583
152	0.228	0.392	0.380	0.260	0.020	0.720	0.158	0.142	0.700
153	0.126	0.384	0.490	0.164	0.011	0.825	0.105	0.122	0.733
154	0.116	0.290	0.594	0.122	0.008	0.870	0.084	0.136	0.780
155	0.176	0.162	0.662	0.119	0.009	0.872	0.079	0.157	0.764
156	0.062	0.258	0.680	0.044	0.008	0.948	0.038	0.066	0.896

X				XI			XII		
	s	e	l	s	e	l	s	e	l
144	0.940	0.044	0.016	0.900	0.088	0.012	0.944	0.054	0.002
145	0.954	0.019	0.027	0.892	0.085	0.023	0.752	0.244	0.005
146	0.928	0.032	0.040	0.828	0.154	0.018	0.718	0.266	0.016
147	0.876	0.051	0.073	0.732	0.239	0.029	0.632	0.321	0.047
148	0.808	0.066	0.126	0.618	0.318	0.064	0.464	0.424	0.112
149	0.729	0.076	0.194	0.496	0.382	0.122	0.267	0.515	0.218
150	0.640	0.086	0.274	0.376	0.426	0.198	0.110	0.530	0.360
151	0.539	0.097	0.354	0.268	0.443	0.288	0.031	0.446	0.524
152	0.426	0.106	0.468	0.184	0.424	0.392	0.020	0.296	0.684
153	0.305	0.109	0.586	0.130	0.362	0.509	0.029	0.157	0.814
154	0.190	0.102	0.708	0.104	0.264	0.632	0.004	0.100	0.896
155	0.111	0.087	0.802	0.087	0.170	0.743	[—] 0.054	0.119	0.935
156	0.120	0.084	0.796	0.030	0.174	0.796	0.000	0.022	0.978

TABLE VI. ANALYSIS OF EQUALITY JUDGMENTS

	n	Mean	Maximum of function	Position of Maximum; $x_m =$	$0.6745 \sqrt{\frac{\sum v^2}{6}}$	$0.6745 \sqrt{\frac{\sum v^2}{n(n-1)}}$
I	656	150.00	0.323	150.33	23.33	0.087
II	541	150.02	0.220	148.28	22.15	0.100
III	688	150.58	0.285	151.60	25.78	0.092
IV	339	150.82	0.192	151.67	16.08	0.117
V	514	149.46	0.204	146.19	22.99	0.110
VI	1022	149.60	0.394	147.30	32.43	0.078
VII	999	150.49	0.396	152.61	31.16	0.077
VIII	135	147.78	0.069	147.48	9.62	0.175
IX	521	149.39	0.212	149.44	22.34	0.105
X	260	151.15	0.111	153.38	15.91	0.150
XI	924	150.63	0.443	150.97	26.31	0.070
XII	846	149.34	0.536	149.65	20.19	0.058

TABLE VII. COEFFICIENTS OF DIVERGENCE

	Shorter	Equal	Longer	Mean		Shorter	Equal	Longer	Mean
I	2.14	3.27	2.09	2.50	VII	1.38	1.41	1.25	1.34
II	1.45	2.37	2.46	2.09	VIII	2.21	1.94	1.82	1.99
III	2.75	1.99	2.53	2.42	IX	1.59	1.73	1.31	1.54
IV	2.19	2.93	2.60	2.57	X	1.68	3.27	1.97	2.31
V	1.75	2.40	2.27	2.14	XI	1.37	1.31	1.38	1.35
VI	2.19	2.02	2.55	2.25	XII	1.35	1.77	1.77	1.62

TABLE VIII. INTERVAL OF UNCERTAINTY

Subject	By Interpolation				By the Method of Just Perceptible Differences					
	Lower limit	Upper limit	Interval	Middle point	Calculated		Observed		Interval	Middle point
					Lower limit	Upper limit	Lower limit	Upper limit		
I	149.17	151.41	2.24	150.29	148.57	150.92	148.71	151.43	2.72	150.07
II	147.34	150.27	2.93	148.80	147.28	150.06	147.41	150.23	2.82	148.82
III	150.35	153.64	3.09	152.09	149.58	147.41	150.94	152.49	1.55	151.71
IV	150.78	152.74	1.96	151.76	150.53	151.10	150.70	151.93	1.23	151.31
V	147.91	150.48	2.57	149.19	146.97	149.97	148.50	150.21	1.71	149.35
VI	144.91	153.51	8.52	149.25	142.96	149.64	146.92	151.07	4.15	148.99
VII	147.68	153.09	5.41	150.38	148.08	148.77	148.77	152.30	3.53	150.54
VIII	149.91	150.27	0.36	150.09	149.53	150.15	149.73	150.24	0.51	149.99
IX	147.83	150.30	2.47	149.06	146.27	149.71	147.19	149.95	2.76	148.57
X	151.35	152.27	0.92	151.81	150.67	150.58	150.95	152.00	1.05	151.42
XI	148.97	152.93	3.96	150.95	148.91	150.07	149.07	152.74	3.67	150.91
XII	147.78	150.85	3.07	149.31	147.50	150.88	146.98	150.92	3.94	148.95
Mean	148.67	151.81	3.12	150.24	148.07	149.94	148.82	151.29	2.47	150.05

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## The Form Board Test

By

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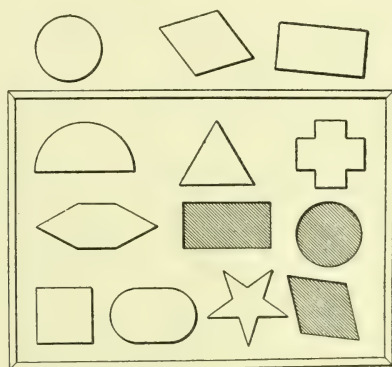
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# I

## DESCRIPTIVE AND HISTORICAL SKETCH

The form board has been used for several years, and clinical psychologists continue to regard it as one of their best general tests. It appeals to the child's interest, affording him a short and fascinating task which calls for his best effort, and it helps to free him from the fear and self-consciousness which often interfere seriously in a mental examination. At the same time the test gives the examiner a good general view of the child's mentality and it usually indicates more or less clearly the nature of his defects.



THE FORM BOARD.—The forms are designated by numbers as follows:  
 1. Semi-circle. 2. Triangle. 3. Cross. 4. Elongated hexagon. 5. Oblong.  
 6. Circle. 7. Square. 8. Flattened oval. 9. Star. 10. Lozenge.

The form board is shown in the above figure. The ten geometrical figures, as nearly uniform in size as their variety of form will allow, are cut through an oak board 20 x 14 x  $\frac{3}{8}$  inches. This oak board is glued to a soft wood board of the same length and breadth,  $\frac{5}{8}$  inches thick. The result is a thick board of moderate weight with a hard oak surface in which the ten forms appear as shallow holes or recesses. About the edge is placed

an oak strip,  $1\frac{1}{4} \times \frac{1}{4}$  inches, fitting flush with the soft wood back and forming a  $\frac{1}{4}$  inch raised edge about the oak surface. Corresponding to the ten recesses are ten walnut blocks,  $\frac{7}{8}$  inch in thickness, each of which fits loosely into its corresponding recess. The thickness being more than twice the depth of the recesses the blocks can easily be grasped and removed. The board and the blocks are finished in their natural oak and walnut colors and the recesses are painted black. The whole is carefully finished in order to give it an attractive appearance,—an important feature in a mental testing device. This description applies to what may be called the standard form board,—the type now in most general use.

#### HISTORY OF THE FORM BOARD

The first form boards were contrived for training purposes. Itard in his efforts to train the Wild Boy of Aveyron used as one of his devices a board two feet square upon which were pasted three pieces of brightly colored paper,—a red circle, a blue triangle, and a black square. Pieces of card board of the same forms and colors were to be matched with these by the boy. Other boards with various forms and colors were also used.<sup>1</sup>

Seguin constructed a number of form boards. Copies of some of them are still used at the Seguin School. One consists of an inch board about one foot square into the surface of which are cut four circular recesses a half inch deep and varying between an inch and three inches in diameter. Corresponding to these are four circular blocks one inch thick. Board and blocks are soft wood and are not stained or painted. Another Seguin board is of hard wood, is considerably larger than the kind just described and has a dozen variously shaped symmetrical forms. In a third kind the blocks are of light colored wood on one side and of dark colored wood on the other. The only form boards that Seguin himself made and used are in use at the Massachusetts Institution for the Feeble-Minded at Waverly. They are about two feet long and less than half as wide. The

<sup>1</sup> Des Premiers Developments du Jeune Sauvage de L'Aveyron, p. 41.

six recesses in each are arranged in a line. Boards and blocks are all of the same wood and color. Seguin conceived of a series of form boards graded as to difficulty and he had such a series planned and partly constructed.

Bourneville recommended light form boards or trays<sup>2</sup> very like those used by Dr. Maria Montessori. Montessori is the first to apply form board devices to the training of normal children.<sup>3</sup>

The form board was first used as a testing device by Dr. Naomi Norsworthy. In her study of mental defectives<sup>4</sup> she used as one of the tests a form board that had been constructed for practice curve studies by Dr. Joseph Hershey Bair.<sup>5</sup> This board was smaller than the standard form board already described, its blocks were provided with handles, and instead of the star and the cross it had a hexagon and an octagon.

Dr. Henry H. Goddard increased the board to its present size, substituted the star and the cross, arranged the forms more compactly, reduced them to such sizes and proportions that no block could be set into a recess not its own, and dispensed with the handles.

Professor Edwin B. Twitmyer adopted Goddard's arrangement and size of forms, but reversed their order, made the recesses shallower, used hard wood, contrasted the colors of board, blocks, and recesses, added the raised strip to the edge, and gave the whole a more attractive appearance. This is the kind of form board that was used in the present investigation.<sup>6</sup>

#### METHODS OF GIVING THE TEST

Dr. Clara H. Town regards form perception as the primary feature of the test and so uses the number of *errors*<sup>7</sup> as the index

<sup>2</sup> Assistance des Enfants Idiots et Dégénérés, p. 233. *Recherches Cliniques et Thérapeutiques*, vol. XXIV, p. xxv.

<sup>3</sup> The Montessori Method (Tr. by Anne George), pp. 195 ff.

<sup>4</sup> The Psychology of Mentally Deficient Children, pp. 25, 26.

<sup>5</sup> The Practice Curve, p. 34.

<sup>6</sup> Robert S. Woodworth (Science, n.s. XXXI:171), and William Healy and Grace M. Fernald (Tests for Practical Mental Classification), have constructed and used other good form boards.

<sup>7</sup> By an *error* is meant an attempt to fit a block into a recess not its own.

of a child's form board ability. She takes a record of the number of errors made in each *trial*<sup>8</sup> until the trial in which all of the blocks are replaced without error, or until she is convinced that the child cannot replace them. In addition she notes the rapidity of the work and certain other features, but her procedure is planned to give greatest prominence to errors.

Goddard considers the amount of *time* required by the child for replacing the blocks as of prime importance. He gives three trials, and takes the time of the shortest of the three as the child's form board index. He also takes a record of the handling of the blocks and attaches some importance to the number of errors.<sup>9</sup>

Professor Lightner Witmer is most interested in the child's first attempts at the task. His procedure varies for different children, but he usually places the board before the child with no explanation except a mere statement as, "Let us see whether you can do this", or "Put the blocks in". Then he watches closely to catch the child's first reactions and to see how he attacks this new kind of problem. Successive trials are usually given and the method varied, the procedure depending on the way that the child reacts and the particular features of his mentality on which the examiner desires more light. If the child takes the usual interest in the task, he is often allowed to continue it while details quite apart from the general purpose of the test are studied. For instance after the blocks are in place the examiner may say in a low tone, "Now take them out", thus getting at the child's word-hearing ability. The record of the test as kept by Witmer usually consists of observations dictated while the test is being given.

These three methods are distinguished because they emphasize three different features of the form board test; errors, time, and reaction to a new task. In each some attention is given to the features emphasized in the others, so they are not entirely distinct. Other methods are modifications of these three.

<sup>8</sup> By a *trial* is meant the taking of all of the blocks from a pile and putting them into their recesses.

<sup>9</sup> Training School, IX, 49-52.

## II

### PRELIMINARY STUDIES

The purpose of the investigation reported in this monograph was to analyze certain features of the form board test psychologically, to determine upon the best method of applying it, and to work out a standard interpretation of its results. A long series of preliminary studies was necessary. Following a year's observation of the test in the Psychological Clinic of the University of Pennsylvania the investigator applied it to some four hundred children and several dozen adults, using various modifications of the three methods mentioned in the preceding section. Interviews with Goddard, Town, Woodworth, Wallin, Mrs. Seguin, and others who have used the test extensively, and conferences with the professors of psychology under whose direction the main investigation was to be carried on made possible a full and satisfactory interpretation of the results. These preliminary studies, besides giving a general orientation and opening up the various lines which would have to be followed out, yielded conclusions on three points which had to be decided tentatively before the investigation could proceed.

#### POSITION OF CHILD, BOARD, AND BLOCKS

The first of these conclusions has to do with the position of the child, the board, and the blocks at the beginning of a trial. The following arrangement was worked out. It was used throughout the later studies and proved to be entirely satisfactory. The form board lies horizontally on a table, its lower edge<sup>1</sup> even with the edge of the table next to which the child stands. The table must be low enough so that he can lean well over the board and look down upon the center of it. Children readily adapt themselves to height within a reasonable range, so an adjustable table is not necessary. One of ordinary height and a kindergarten table suffice, most children under nine years

<sup>1</sup> The lower edge is the edge next to the star recess.

of age requiring the latter. If the table is too high, the child has to look across the board instead of down upon it and he cannot see the forms so well;—an important point that is often neglected, many examiners having the board entirely too high for the child. The blocks should be placed in three piles on the table, next to the edge of the board on the side opposite the child, no block being in the pile nearest its own recess. If the child is in a position that enables him to look down upon the center of the board, he can easily reach the blocks piled in that way. Placing them at the right of the board as is often done, is of no advantage, and in that position they cannot be picked up with the left hand. Placing some at each end of the board is still worse for it offers the most possibilities for varying the difficulties of handling them.

#### KIND OF FORM BOARD TO BE USED

The second conclusion referred to the size of the board and the order of arrangement of the forms upon it. Some have suggested that the blocks of the standard form board are too large for small children. To test this a two-thirds sized model of the standard board was constructed. This board was tried with 15 six year old children, 28 five year olds, 18 four year olds, and 8 three year olds. Each child had two trials with the standard form board and two with the small one, half of each age taking them in the order, standard-small-standard-small and the other half taking them in the reverse order. The time required for placing the blocks was found to be practically the same for the two boards. The small board has a slight advantage in that small children can reach the extreme corner recesses more easily, but this is perhaps more than offset by the finer co-ordination required for fitting the small blocks into place. The small star was very difficult for the clumsy fingered little folk. The investigator and others who observed the work agreed that the regular sized blocks were grasped and handled with more certainty than the small ones. It was not thought worth while to try a larger board for it was evident that small children would

have difficulty in reaching its corner recesses. The question of re-arranging the forms on the board and of substituting other forms was also taken up. A board on which forms could be set in any order and turned at any angle was planned, but after experimenting with cardboard models it was decided that such a study would involve more than the present investigation should undertake; and further that the study of these details would probably contribute little to the efficiency of the device. It is obvious that the value of a test like this depends less on fine details of devices than on the method of using them and the interpretation of their results. It was therefore decided to proceed with the investigation using the standard form board.

#### PRELIMINARY CONSIDERATION OF METHOD

The third point which had to be decided tentatively before the investigation could proceed was as to which of the three methods should be the basis of the one used. Witmer's method, because it leaves the examiner free to fit the procedure to each individual case, brings out features of the child's mentality which the other methods cannot. But it is not adaptable to a quantitative study of groups of children such as was contemplated, and its results cannot be readily reduced to standards for comparing and ranking individuals. Town's method is more truly a form perception test than the others, but preliminary studies showed that normal children make so few errors that their records promise little in the way of norms and standards.<sup>2</sup> Goddard's method prescribes a definite procedure which partly prevents getting the most out of the first trial, but it gives quantitative results and makes possible the establishment of norms and standards. For this reason it was unquestionably the method to serve as the basis of the intended investigation.

#### NUMBER OF TRIALS TO BE GIVEN

After these first preliminaries had been completed attention was given to a feature of Goddard's method which seemed to call for testing before being adopted, namely, the giving of three

<sup>2</sup> See chart III (page 36), and page 51.

trials. At the beginning it was necessary to set age limits for the children to be tested. Records had been kept of the 400 children and a number of others of children from three to seven years of age were now added. The results showed that an occasional four year old child could not place all of the blocks unless given assistance other than urging. So five years was set as the minimum age for the establishment of standards. Fourteen years was set as the maximum age because the form board is certainly of little value for testing individuals who have the ability of that age or of a year or two younger. The question of the number of trials was taken up by testing 200 children, 20 of each age from five to fourteen inclusive. Each child was given five trials at placing the blocks and the time of each trial was recorded. The results arranged in two year groups are given in tables I, II, and III. According to table I there is a

TRIAL	AGE					Average
	5-6	7-8	9-10	11-12	13-14	
I	45	29	22	18	15	25.8
II	34	24	18	16	14	21.1
III	31	23	17	15	13	19.6
IV	30	21	18	14	13	19.2
V	30	22	17	13	12	18.9

TABLE I.—Average time in seconds for each of five trials. The data are from the records of 20 children of each age from five to fourteen.

TRIAL	AGE					Average
	5-6	7-8	9-10	11-12	13-14	
I	13.8	5.5	5.1	4.7	2.6	6.3
II	11.0	5.2	3.4	2.8	3.0	5.1
III	9.5	3.5	3.2	2.5	2.3	4.2
IV	7.8	3.8	3.1	2.5	2.1	3.9
V	7.6	3.1	3.3	2.4	2.2	3.7

TABLE II.—Standard deviations for the data of table I.

TRIAL	AGE					Total
	5-6	7-8	9-10	11-12	13-14	
I	1	0	2	1	2	6
II	8	9	9	8	6	40
III	11	12	14	10	12	59
IV	13	11	13	19	12	68
V	14	17	19	20	18	88

TABLE III.—Number of individuals making their shortest record on the first trial, on the second trial, etc. for the five trials. Data of table I. Where the shortest record was made on two different trials each is credited with it.

general decrease in the length of time records of successive trials, the average falling from 25.8 seconds for the first trial to 19.6 seconds for the third trial. Each age group shows the decrease regularly for the first three trials. The decrease for the fourth and the fifth trials is not so marked, the time averages being 19.2 seconds and 18.9 seconds respectively, and in some of the groups the decrease is not regular. Variability (Standard deviations, table II) also shows a decrease with successive trials, the averages of the five in order being 6.3, 5.1, 4.2, 3.9, and 3.7 seconds. Here also the decrease is greatest in the first three trials and the age groups show regular decreases except in the fourth and fifth. Table III indicates that practice is a very important factor, most of the shortest records being made after the second trial, and a larger number on the fifth trial than on any other. This evidence has less weight when considered in the light of the small average time decreases for the fourth and the fifth trials as has been noted in table I, for with such small average decreases, it must have been that in a great number of cases the last trials were shortest by only a second or two. These three tables indicate that in general the first trial is the most irregular in every way and so is the least reliable. Likewise the fifth trial is the most reliable, and of the five trials each is more reliable than those preceding it. The third trial is so much more consistent than the first and the second that the necessity of giving at least three trials is obvious. But the differences between the third, fourth, and fifth are comparatively small and as will be shown farther on, a difference of a second or two in indices is of little consequence. It is evident then that the demands for brevity and convenience in a test like this more than offset the small gain in accuracy that would be made by giving a fourth or a fifth trial. Therefore the adoption of three trials for the standard method is justified.

#### POSITION OF THE BLOCKS IN THE THREE PILES

Another preliminary study was the testing of 93 totally blind children in the Pennsylvania Institution for the Blind. Certain

features of the test stand out more clearly in the work of the blind than in the more rapid and less labored work of those who see. One feature observed was that when two difficult blocks or two that are often interchanged are picked up by the two hands at the same time, it is likely to confuse the child and to prevent his making the best record of which he is capable. The star and the cross are the most often interchanged by the blind and the lozenge and the enlongated hexagon by seeing children. This observation led to the rule that in piling the blocks for children who have vision the lozenge and the elongated hexagon must not be placed in the same layer in the piles. This usually prevents their being picked up simultaneously. It was also observed especially in the blind that if the star is picked up early in the trial and refuses to slip into place the child is often confused thereby and has unnecessary trouble with the other blocks. It was therefore decided that this, the most difficult block to fit into place, should never be left on the top of a pile. If picked up late in the trial it cannot disturb the handling of so many other blocks.

#### RELATIVE IMPORTANCE OF TOUCH AND VISION IN THE TEST

The main purpose in testing the blind children was to get further evidence as to the relative importance of the visual and the tactual senses in the form board test. In spite of the fact that the child gets no tactile impression of the recesses while placing the blocks, it is the opinion of some examiners that touch is depended on considerably by children who see. Careful observation however, has shown that they usually pick up the blocks with no effort to get a tactile impression of them. In the tests with the smaller board no advantage was taken of the clearer tactile impressions which the smaller blocks must have given. Introspective reports of students of psychology who were given the test indicate that there is little dependence on touch. Some blindfolded children are unable to place the blocks at all, and blindfolded adults have great difficulty, requiring on an average about three minutes for the first trial. Table IV shows the

	Number of individuals	Average age	Average time in seconds	Average number of errors
Blind from birth	31	13	69	4.3
Vision lost before the age of three	32	15	53	3.8
Vision lost between the ages of three and ten	22	14	37	1.4

TABLE IV.—Results from form board tests of totally blind children.

records made by the blind. At the beginning of the test the child explored the board with his hands, examining every recess and handling its corresponding block. He was then given three trials, each of which was timed and a record was taken of the number of errors. The data given in the table are from the shortest of the three time records and the number of errors made in that trial. It might be expected that those who have been blind from birth would be the most successful in the test because of having always depended on the tactile sense instead of having adapted themselves to it after form and position had been learned visually, but the results do not fulfill this expectation. Those who had been blind from birth required the longest time for placing the blocks, an average of 69 seconds, while those who had retained their vision until after the age of three required on the average only 39 seconds. The average number of errors made by the two groups were 4.3 and 1.4 respectively,—further evidence of the difficulty of the test for those who had been blind from birth. Obviously they were hindered by something or else those who had visual experience were helped by something. The small age differences could not have provided the factor. Since the three groups differed in no other way, the better success of those who had had visual experience must have been due to something that they retained from it. The conclusion must be that they retained their visual imagery and were assisted

by it in the interpretation of their tactile impressions. The fact that those who lack visual imagery find the form board test so difficult indicates that vision is much more important than the tactile sense in the test; in fact this evidence added to that from observations and from introspections of normal subjects leads to the conclusion that the tactile sense is an almost negligible factor in the form board test.<sup>3</sup>

#### SUMMARY

The conclusions from these preliminary studies have been reported on the preceding pages in the order in which they were reached. In the following summary they are more conveniently grouped.

1. Without a long and elaborate series of experiments (probably not worth while), one could not improve on the size, arrangement, and choice of forms as they appear on the standard form board.

2. In the test, the form board should lie horizontally on a table which is low enough to allow the child to lean over and look down directly upon the center of the board. The blocks should lie in three piles at the top of the board, with no block in the pile nearest to its recess, the lozenge and the elongated hexagon in different layers, and the star not at the top of a pile.

3. Goddard's method or a modification of it is the most promising for a quantitative study and for the establishing of norms and standards for comparing and ranking individuals.

4. This method cannot be standardized for children younger than five years of age because some of them cannot place all of the blocks without help other than urging. It is not worth while to establish norms for those above fourteen.

5. The tactile sense figures very little in this test.

<sup>3</sup> Fernald, *Psychological Bulletin*, X, 62; Sylvester, *Psychological Bulletin*, X, 210; Dearborn, *American Journal of Psychology*, XXIV, 204.

### III

## A STUDY OF THE FORM BOARD TEST IN ITS APPLICATION TO RETARDED AND DEFECTIVE CHILDREN

The first important study following the preliminary work was the testing of the children in the special backward classes of the Philadelphia Public Schools. At that time there were 45 of these classes with a total enrollment of about 780. Of this number some were foreign born children placed there until they could get a start in English, some were there for disciplinary reasons, and some because of deafness, poor vision, or other physical defects. These three groups were not included and a few other children were absent from school when the tests were made, so the total number tested was 616. The ages of 11 of these were not obtainable so their records were thrown out, leaving 605. Goddard's method was used, modified as to the piling of the blocks and in other ways to accord with the conclusions drawn in the preliminary studies. In addition the child was to be graded on as many features as possible. The teacher's estimate of the child and any other information that she could give concerning him were also to be used. The work was undertaken with three purposes; first, to determine which features of a child's work at the form board can be satisfactorily graded; second, to find which of the obtainable facts concerning him are of value in connection with the test; and third, to differentiate the characteristic ways in which children of various types work at the test. The first two of these purposes were successfully carried out but the third was not, the 605 children proving to be such a heterogeneous group and the data so inco-ordinate as to defy all attempts at classification. The work had an additional value in serving as a preparation for the more careful quantitative studies of normal children. Improved ways of securing proper testing conditions were developed with experience and the procedure of the test itself was adjusted and smoothed.

## PLAN AND PROCEDURE

At the beginning the test was explained to the child quite fully, and during the explanation the examiner put all of the blocks into place and removed them once. As it had been decided to make the time element the main feature it was thought that the child should be given every chance to make his best possible record. (For a better procedure that was worked out later see page 34.) The child started each trial from the signals,

FIRST TRIAL	SECOND TRIAL	THIRD TRIAL
686	939	9
9	725867	6
3	2	5
01050	81508	383
8548	515	1
141	3	4
54	606	7
4	0	828
2	1	020
7	4	2
5		
—	—	—
61 sec.	77 sec.	49 sec.

“Ready—Go.” The records of the handling of the blocks were taken by an assistant in the form shown in the accompanying chart. This specimen record shows that the child began by picking up block 6, trying it at recess 8, and then placing it in its proper recess. (See page 1 for form numbering.) Next blocks 9 and 3 were placed correctly. Block 0 was tried at recess 1, then unsuccessfully at its own recess, then

at recess 5, and finally it was fitted into its own recess. Two errors were made with block 8 and one with block 1. Block 5 was tried at recess 4 and laid aside, then blocks 4, 2, 7, and 5 were placed in order. Thus the handling of every block in the first trial is shown. At the foot of each column is recorded the time of the trial in seconds.<sup>1</sup>

The investigator besides handling the stop watch recorded his estimate on the child's co-ordination, apparent mentality, ability at planning ahead, and use of the hands. From the teacher were obtained data including the child's age, reasons for his being in the special class, whether she regarded him as mentally defective or as merely retarded, his general school progress, and her estimate of his ability at hand-work. At the beginning the investi-

<sup>1</sup>This is Goddard's method of taking the record.

gator undertook to estimate certain other features such as interest, attention, alertness, and learning ability, but one by one they were dropped as it became evident that they could not be estimated in such a way as to have a bearing on the test. After some 200 children had been tested, it was evident that there was another feature which should have been included, namely, poise. The remainder of the children were graded on this. Exactly what is here meant by poise is made clear in the discussion of results (page 19).

### AGE AND SEX CONSIDERATIONS

After various attempts had been made at arranging the data it became evident that the time records have the most consistent variability and are therefore the best basis for arrangement. The grouping above the 18 second records in table V is more or less forced but it is the least objectionable of any that were tried. In the first columns at the left are shown the number of individuals in each time record group, their distribution by ages, and their average ages. Even these sub-normal children show some correlation between age and the time required for placing the blocks. In the column of average ages there appears a gradual increase of age from the 40-49 second group to the 10 second group, but the distribution shows that the shortest records were made not by the oldest but by the fourteen year old group. The shortest records focus toward that age. Arranging the data in a way not shown in the table it is found that the average time record for each age is as follows:

Age	7	8	9	10	11	12	13	14	15	16	17
Av. time	22.6	23.7	20.9	19.4	19.1	17.5	16.6	15.0	16.8	16.5	16.6

The fact that the fourteen year old group made shorter records on an average than the older ones is due to the brighter children dropping out of school after the age of fourteen, which is the limit of compulsory education. Why this elimination is selective, leaving the less capable individuals in the special backward classes, is not pertinent to this study.

Sex distribution is of little importance. For reasons not of

Time in Seconds	Number of Individuals	Distribution by ages											Average age	Sex	
		7	8	9	10	11	12	13	14	15	16	17		Male	Female
Unfinished	6	I			2	I	I			I			10.8	4	2
50-101	15	I			3	3	3	2	I	2			11.7	7	8
40-49	21	I	10	2	2		3	I	I		I		9.7	7	14
30-39	24	I	3	4	5	5	I	2	2	I			10.5	15	9
26-29	33	I	5	2	6	8	4	3	2	I	I		10.8	22	11
23-25	41	I	4	8	5	11	6	I	2	2	I		10.7	27	14
21-22	46		I	5	8	9	6	6	5	4	I	I	11.8	27	19
19-20	60		2	8	8	10	12	7	5	6	2		11.7	49	11
18	61		I	3	5	13	22	8	6		3		11.9	51	10
17	49	I		2	I	12	12	9	7	4	I		12.3	35	14
16	68			3	13	8	11	16	7	7	3		12.3	61	7
15	60				3	6	8	17	14	4	7	I	13.3	52	8
14	62				I	7	13	9	16	9	6	I	13.4	54	8
13	31				I	2	7	4	13	3	I		13.3	28	3
12	15						I	5	7	2			13.7	15	
11	10						I	I	7	I			13.8	10	
10	3								3				14.0	3	
Total	605	7	26	37	63	95	111	91	98	47	27	3		467	138

TABLE V.—The data from the 605 backward class children.

interest here, a relatively small number of girls are placed in the special backward classes. It is a matter of observation confirmed by these results that the girls of these classes, as a group, are more backward than the boys. The table shows that the shortest form board records were made by boys entirely. The average for all records was 20.3 seconds for boys and 26.2 seconds for girls. Obviously the girls of a mental grade corresponding to the brighter boys in the backward classes were left in the regular classes. If equal numbers of boys and of girls were selected for the special backward classes they would be more nearly of the same grade of mentality and their form board records would be more nearly equal. Later form board tests of normal children revealed no sex differences.

#### THE TIME RECORDS IN RELATION TO SCHOOL WORK ABILITY AND TO MENTALITY

The main purpose of the columns on school progress, hand-work, and mentality is to give the reader information concerning

School progress			Hand work			Mentality			Using both hands to place blocks	Very poor in coordination	Seriously lacking in poise	Planning the work ahead	Average number of errors
Fair	Poor	Very poor	Fair	Poor	Very poor	Retarded	Doubtful	Defective					
		6			6			5		1			
		15			14		2	13		7			36
	2	19		1	21	1	4	10	1	12			17
	5	19			24	2	4	18		10	2		15
1	6	26		4	29	5	10	18	4	9	9		9
1	10	30	3	6	32	12	10	19	5	11	9		7
2	6	38	1	9	36	10	15	21	7	12	8		5
3	18	39	4	16	40	23	13	24	9	15	13		6
	12	49	6	17	38	18	22	21	12	12	10		5
2	6	41	5	15	29	23	8	18	14	12	1		5
5	16	47	8	18	42	34	9	25	20	13			3
2	13	45	6	23	31	32	12	16	13	14		6	4
1	10	51	17	16	29	40	12	10	25	2		8	3
5	7	10	6	6	19	19	7	5	21	3		12	4
3	3	9	2	3	10	14		1	14			13	3
1	3	6	3	4	3	7	1	2	10			10	3
2	1		2	1		3			3			3	3
3	118	459	63	139	403	243	129	233	158	133	57	52	

TABLE V.—Concluded.

the kind of children that were tested. The grading calls for explanation. School progress and hand-work were reported by the teachers on a three point scale: fair, poor, and very poor, the standard being that of ordinary school children. Aside from showing a much stronger correlation between hand-work ability and form board ability than between school progress and form board ability, the data contributes little except that it helps to give a notion of the personnel of the groups of children.

Unfortunately no good estimate of the grade of mentality of each child could be made. Had they been graded or grouped according to some approved scheme of mental classification it would have aided greatly in the interpretation of the form board results. The best that could be done was to record the investigator's estimate of the child's mentality after he had watched him through the form board test and perhaps asked him a few questions. There was also recorded the teacher's estimate based on her impression of the child as her pupil. A two point grading was adopted; all being graded either as *retarded* or *defective*.

The criterion was that those placed in the lower group, the defectives, had evidently been subnormal from birth and could never have been trained to economical and social independence. This classification is far from satisfactory and it involves a further misuse of the already over-worked terms, *retarded* and *defective*, but it served fairly well for a common basis for estimates by teacher and investigator. In cases where the two agreed there was some likelihood of their being correct. The middle column, marked *doubtful*, contains 129 cases on which the opinions of the examiner and the teacher disagreed. It is unfortunate that this number should be so large, but it is likely that most of them were borderline cases or cases not easily understood. The distribution in the *retarded* and the *defective* columns indicates that the former group did the test much the more successfully. The average time records for the two groups were 16.5 seconds and 30 seconds respectively. Although the grouping is no doubt faulty, there is certainly strong evidence here of a correlation between mentality and ability at the form board test.

#### IMPORTANT FEATURES OTHER THAN TIME AND ERRORS

The four columns next to the last in table V give the data which it was found can be taken in connection with the form board test and which contribute to the value of the test in diagnosis. First is shown the number of children of each time record group who used two hands successfully and simultaneously in placing the blocks. As compared with normal children (see page 50) a relatively small number did this. One feature observed but not shown in the table is that several of the older children who used but one hand at a time, changed from one to the other in successive trials, apparently succeeding with one as well as with the other. Normal children rarely change hands.

In muscular co-ordination 133 were graded as very poor. Inco-ordination is not so noticeable in children whose mentality is such that they attempt no quick or accurate movements, so these results do not mean that all but 133 of these 605 had good co-ordination.

*Poise*, as here used, means the ability to work at one's maximum speed without losing control and getting confused. When a child in his efforts to place the blocks quickly, over hurries and gets flustered so that he makes numerous and inexcusable errors or hesitates in a semi-dazed way, he does so because he is lacking in this quality which we have chosen to call poise. Take for instance one of these backward cases, an eleven year old boy whose record for the three trials in order were 36, 52, and 62 seconds, and the number of errors 4, 5, and 11. His efforts at hurrying caused him to make errors and to lose time. When given a fourth trial and told to work slowly he placed the blocks in 21 seconds and made no errors. Some defectives show a lack of poise as soon as they begin to work rapidly. Urging by the examiner is likely to throw them into confusion. Later studies of normal children showed that although they are sometimes momentarily hindered by over hurrying, they do not go into utter confusion. Practically all of them make better records when urged by the examiner during the work. In other words, the child who is lacking in poise is very likely not of normal mentality. As previously stated, no records were kept of this factor until the children in several of the classes had been tested. Of 377 who were marked on poise, 57 were graded as seriously lacking in the quality. (Table V.) Many of these 57 were of the excitable defective type; others could not be called defectives but they were mentally retarded because of nervous trouble. Many of them made numerous attempts to fit blocks into wrong recesses, the average of the 57 being 7.3 errors each. Poise is a detail which the examiner can observe to advantage. It is important not only in extreme cases, but in the many who momentarily lose control or show a tendency to do so there is often some instability that calls for further study.

By *planning ahead* is meant that before the signal "Go", the child glances at the blocks on the top of the piles, then at their recesses and is thus ready at the signal to shoot them into place without hesitation. Most normal adults and many children do this (See page 50) but younger children do not. Only 52 of

these backward class children did so, according to table V. An individual is credited with planning ahead if he does it on one or more trials.

### THE RECORDS OF ERRORS

In the last column of table V is shown the average number of errors made by each individual in all three trials. For the extremely long time records the average number of errors is 36, for the shortest records the average is 3, and between these extremes there is a somewhat irregular correlation between the length of time record and the number of errors. These 605 backward children averaged more than 6 errors each, whereas normal children average less than three (chart III, page 36). Evidently a large number of errors indicates low mentality.

A statement of the number of times that each possible kind of error was made is given in table VI. Horizontally the spaces

	Recesses										Total
	1	2	3	4	5	6	7	8	9	0	
1	4	6	3	62	81	3	4	63	1	15	242
2	20	2	24	32	19	9	14	12	9	75	216
3	7	16	4	3	9	18	16	7	62	42	184
4	38	3	9	4	102	3	6	31	4	99	299
5	23	4	2	45	4	5	17	58		4	162
6	9	5	8	6	12		55	67		31	193
7	21	17	23	15	65	67		68	4	97	377
8	111	4	5	61	159	24	9		1	35	409
9	2	19	110	4	10	16	3	4	7	20	195
0	41	60	15	305	116	8	27	61	8	2	643
Total	276	136	203	537	577	153	151	371	96	420	2920

TABLE VI.—Distribution of the kinds of errors made by the 605 backward class children. The upper line, for instance, indicates 4 futile attempts to fit block 1 into its own recess, 6 attempts to fit it into recess 2, 3 at recess 3, 62 at recess 4, etc., and a total of 242 errors with this block. Since each of the 605 children had three trials, a total of 1815 errors with each block was possible. (See footnote, page 23.)

represent the ten recesses of the form board and vertically they represent the ten blocks. The numbers in the upper horizontal line show the number of futile attempts at putting block 1 into each of the ten recesses. Four attempts at its own recess failed and there were six attempts at recess 2, three at recess 3, sixty-

two at recess 4, and so on for the others. The other horizontal lines give corresponding data for the other blocks.<sup>2</sup> According to this table, by far the most frequent error was that of attempting to put block 0 into recess 4. The only possible errors not made were 5-9 and 6-9 and futile attempts to fit block 6, 7, and 8 into their own recesses.<sup>3</sup>

One important conclusion is to be drawn by arranging the data in the form of table VII. Here the twelve most frequent errors<sup>4</sup>

Time in Seconds	Kind of Errors											
	0-4	8-1	8-5	6-8	7-8	4-0	1-4	3-9	9-0	6-7	0-5	9-3
30 to 101	4	3	4	2	2	2	2	3	3	2	2	4
20 to 29	11	4	7	3	3	4	2	2	4	3	5	6
15 to 19	15	4	6	2	3	5	2	2	3	1	5	3
10 to 14	16	5	7	2	1	5	3	2	3	2	4	2

TABLE VII.—The twelve most frequent kinds of errors of the 605 backward class children arranged according to the time records. The data is in per cent. of the total number of errors made by each of the four time record groups. Thus, the 4 in the upper left space means that of the total number of mistakes made by the group whose time records were 30 seconds or more, 4 per cent. were the 0-4 error.

are arranged according to four time record groups,—those longer than 29 seconds, the 20 to 29 second records, the 15 to 19 second records, and those shorter than 15 seconds. This is a condensation of the grouping that is used in table V. The data are given in percentages of the total number of errors made by each group. In table XIV, page 40, it is shown that with normal children of all ages the 0-4 error is by far the most frequent and that the

<sup>2</sup>In this enumeration of kinds of errors, all three trials are included but only the first wrong recess tried with each block. For example, from trial I in the record shown on page 14 there was taken only the 6-8, 0-1, 8-5, 1-4, and 5-4 errors. The failure to fit block 0 into its own recess and the attempt with this block at recess 4 are not included. This is necessary because only the first error is made directly after the child has looked at the block in the pile and glanced over the board for its recess. The errors after this first one are made under various conditions and so do not merely represent a failure to perceive the relation of block to recess.

<sup>3</sup>Where the kind of error is designated by two numbers separated by a dash, the first number names the block and the second the recess.

<sup>4</sup>The twelve most frequent errors are almost the same in tables VII and XIV. They are arranged here according to their frequency in the latter, in order that the two tables may be compared.

occurrence of the more common ones does not vary significantly with age. In table VII the same is true of the two groups whose time records average below 20 seconds and to a less degree of the 20 to 29 second group, but in the longest records group there is little tendency to make one kind of error more frequently than another. Since nearly all of these longest records were made by children of quite low mentality, the one conclusion to be drawn is that if a child makes the 0-4 error and the other common ones more frequently than others he is to be *credited* for doing so. In other words, he is probably of higher mentality than a similar child whose errors are more evenly distributed. This feature is peculiar in that it varies with the degree of mentality but not with the age and it is therefore especially important.

#### SUMMARY

This study of retarded and defective children yielded the following conclusions:

1. Those children whose time records were the longest are generally of the lowest mentality.
2. It is impracticable to record observations on interest, attention, alertness, and certain other features in a regular manner. In cases where they are important they must be recorded in the examiner's general notes or in connection with other tests and parts of the examination. Muscular co-ordination and poise are splendidly revealed in the form board test and are well worth grading, and records should be made of whether two hands are used at the same time successfully and whether the child plans ahead.
3. Records of the handling of the blocks can be satisfactorily taken and are of great value. The greatest number of errors occur in the long time records and are made by children of low mentality.
4. Some kinds of errors are more common than others. A tendency toward making more of certain errors than of others indicates higher mentality than does a tendency to make one error as often as another.

## IV

### A SPECIAL STUDY OF THE TIME AND ERROR FEATURES OF THE FORM BOARD TEST

The most serious difficulty to be met in the study of age variations in the results of any test lies in the differences of advancement of the individuals in each age group. Every child is more or less retarded or precocious, or both. Not only may his physiological age, his mental age, his pedagogical age, and his chronological age be at variance with each other but there may be a wide range of variation within each except the last. For instance, a child ten years of age pedagogically (that is, in fourth grade at school) may be well advanced in reading but very backward in writing or arithmetic. Physiologically he may have the muscular co-ordination of a twelve year old but only the muscular strength of a child two or three years younger. Mentally he may pass the digit memory test of the twelfth year Binet questions but fail on the ninth year definition question. Excluding defectives and other noticeably peculiar individuals, one still has in ordinary children of a given chronological age, a most heterogeneous group. The ages are scattered over the entire year so that an eight year old child may be 360 days older than certain others of the eight year old group but only a day or two younger than some of the nine year old group. To smooth out such variations and to obtain reliable results in a study of age changes requires a huge mass of data, the collecting of which is impracticable in a study such as this one with the form board. Under the most favorable circumstances only ten to fifteen children can be tested in an hour. In the present investigation the difficulty was partly met by testing strictly limited groups of children, selected according to requirements which partly eliminated the factors causing the heterogeneity. Reasonably extensive data from

groups as nearly homogeneous as careful selecting could make them were collected.

#### SELECTION OF THE CHILDREN

Five hundred children were tested; 25 boys and 25 girls of each age from five to fourteen inclusive. Reasons for these age limits have been given on page 9. The requirements were as follows:— (1) The birthday of each child selected came within a month of the day on which the test was given. This made the ages nearly exact by years. (2) He was neither retarded nor accelerated pedagogically according to Philadelphia Public School standards. That is, the fourteen year olds were selected from the eighth grade, the thirteen year olds from the eleventh grade, and so on down to the eight year olds from the second grade. On this scale seven year olds would have been taken from the first grade and there would have been no grade for six year olds. The best that could be done was to select seven year olds from the upper first grade and six year olds from the lower first grade. Five year olds were selected from the kindergarten. (3) Each child was American born and his parents' name and occupation indicated nothing in race or in home conditions especially favorable or unfavorable. Colored children were excluded. (4) He was free from physical defect and there was nothing peculiar or striking in his personal appearance. (5) Mentally he was not especially bright or dull or in any way different from ordinary children.

The method of selection was as follows: The investigator took from the school records the names of children meeting the first three of the above requirements. Principals and teachers checked off from these lists the names of those who in their opinion did not meet the fourth and the fifth requirements. Finally, when the children appeared at the testing room the investigator rejected those whose personal appearance led him to suspect and physical or mental peculiarity. This was the final elimination. All children who were admitted to the test were allowed to complete it and no records were thrown out. The elimination by these requirements was heavy, the records of some 11,000 chil-

dren being gone over before 500 meeting the requirements were found.

A difficult part of the work was the securing of conditions favorable to the children's assuming the proper attitude toward the test. As compared with the carefully controlled conditions of most experiments in the psychological laboratory, it is almost presumptuous to report as psychological tests, work done in a public school and especially by an investigator who is a stranger in the school. If one, however, keeps in mind the ideal of psychological laboratory conditions and does not allow himself to proceed when conditions are not at the best, he is well repaid for it in the reliability of his results. First it is necessary to secure the good will and the co-operation of principals and teachers. If they are impatient and not interested the children will not do their best. Then the children must be dealt with tactfully. Some older boys and girls are inclined to regard the tests as too childish for them, and the little folks are likely to associate it with medical inspectors, throat examinations, and vaccinations. From the experience gained in the preliminary tests and in the tests in the backward classes, there had been worked out a definite plan of procedure which reduced the disturbance of the school to a minimum, usually secured the hearty co-operation of principals and teachers, and put the children into the proper attitude toward the investigator and the test. In a few cases after the work had been begun in a school, it was postponed or abandoned because of some disturbing influence or lack of co-operation on the part of the principal. No tests were given under unfavorable conditions.

The testing procedure was that described on pages 14 and 15, except that the investigator himself took no data. He held the stop watch and otherwise gave his attention to the management of the test.

#### REDUCTION OF THE RECORDS TO TIME INDICES

The data thus collected consisted of individual records of the 500 children showing the time required for each of the three trials and the order in which the blocks were handled, with occasional

observations dictated by the investigator at the end of the test. The first problem to be taken up in the study of the time records was that of reducing each individual's record to a significant value which would stand as an index of his form board ability. In the preliminary studies it had been found that the time of the third trial would be a more reliable index than the time of either of the others. (Page 9.) And according to the usual procedure in psychological tests, especially where practice is so strong a factor, the time of the third trial would be taken as the index. But Witmer's emphasis on the importance of the child's first efforts suggests the use of the time of the first trial as the index, and he would probably record this if he were to keep a time record. Woodworth also favors the use of the first trial record. But the evidence in the preliminary studies was that this trial's results are too irregular to be reliable. Goddard takes the shortest of the three trials for the index. Whipple<sup>1</sup> and Franz<sup>2</sup> use this shortest of three trials index in some of their strength tests. A fourth method of scoring suggests itself,—taking the average time of the three trials as the index. This would include the third trial, the first trial and the shortest trial, giving weight to each.

The distribution of records arranged according to each of these four standards is shown in tables VIII, IX, X, and XI, and their curves of averages of time records for each age are shown in chart I. It is quite remarkable that the four curves run so nearly parallel. So far as is shown in the curves themselves, any one of them could be used as the standard index without serious error, the time averages for the different ages varying in about the same way in all. But variability of individual records indicates that these four standards applied to individual cases would give very different rankings. Take for instance two of the fourteen year old records. A's record is 14, 11, and 9 seconds for the three trials, and B's record is 9, 11, and 10 seconds. Now with the shortest trial as the standard for an index A and B did

<sup>1</sup> Manual of Mental and Physical Tests, pp. 71, 75, and 80.

<sup>2</sup> Mental Examination Methods, p. 49.



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TABLE IX.—Distribution of the *third trial* time records.

Time in seconds.	5	6	7	8	9	10	11	12	13	14
83	1									
71	1									
50										
49	1									
48	1									
47										
46										
45										
44										
43	1									
42	2									
41	1									
40										
39	1									
38	1									
37	3									
36	3									
35	3									
34	2	1								
33	1									
32	1	2								
31	4	2								
30	1	1								
29	5	2								
28	2	2								
27	5	2								
26	5	3	1							
25	4	3	1							
24	1	6	3	1						
23		6	4	3						
22		9	4	4	1					
21		4	14	7						
20		3	5	7	3					
19		1	7	6	9			1		
18			6	9	10	2	1			
17		1	3	7	11	8				
16		2		2	10	9	5	1		
15			1	3	4	9	7	3	3	
14				1	1	8	19	8	3	3
13			1		1	9	10	13	9	3
12						3	4	12	12	9
11						1	1	9	12	17
10						1	2	3	7	12
9							1		4	6

TABLE X.—Distribution of the *shortest trial* time records.

Time in seconds.	5	6	7	8	9	10	11	12	13	14
103	1									
79	1									
62	1									
50	4									
49										
48	2									
47										
46										
45										
44	2									
43	1									
42										
41	2									
40	4		1							
39	2	1								
38	4									
37	1									
36	1	2								
35	4	1								
34	1	5	1							
33	2	1								
32	2	1								
31	4	1	1							
30	2	2								
29	6	6								
28		3	2		1					
27	3	3	2	1						
26		4	7	2						
25		5	4	3	1					
24			7	4	2					
23		4	6	4	1					
22		8	1	5	3					
21		1	5	9	7	3				
20		1	7	6	8	1		1		
19			2	5	7	3	1			
18		1	2	6	8	8	2			
17			1	3	7	10	8	1		
16				2	3	6	9	4	4	1
15					2	11	13	7	5	4
14			1			5	9	13	9	5
13						2	3	12	13	14
12						1		7	10	14
11							3	4	7	8
10								1	1	3
9									1	1

TABLE XI.—Distribution of the *average of three trials* time records.

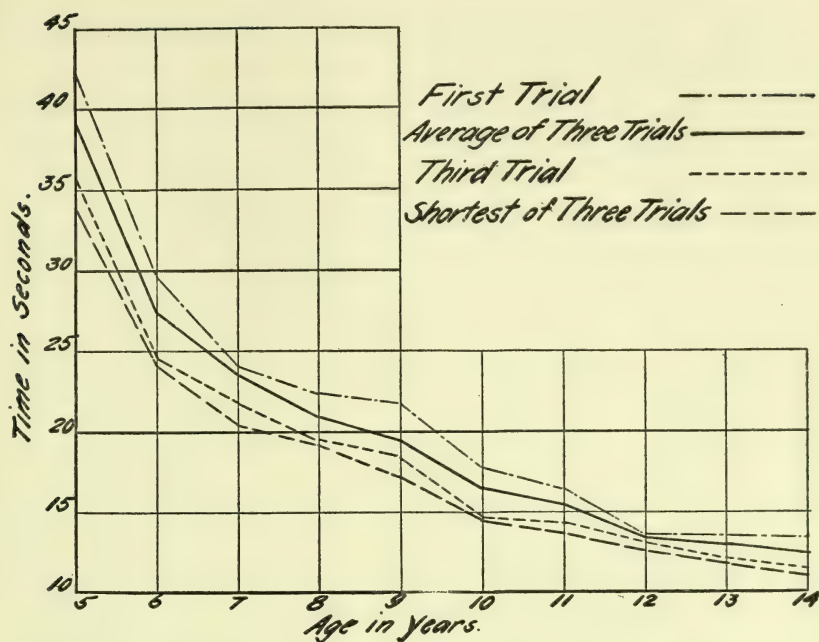


CHART I.—Averages of the time records of the 500 selected children.

the test equally well, the index for each being 9 seconds. With the third trial as the standard A did better than B, the indices being 9 and 10. With the first trial as the standard B did better than A, the indices being 9 and 14. With the average of three trials as the standard B did better than A, the indices being 10 and 11. These are extreme cases but they emphasize the importance of choosing the right one of these standards.

Since variability is the great disturbing factor, that standard which gives the lowest and most regular variability is probably the best of the four. This criterion immediately eliminates the *first* trial standard, chart II showing that its variability curve is both higher and more irregular than the other three. The curve for the *third* trial's variability is fairly low and smooth but this trial as a standard is eliminated by the results shown in table XII. This table shows that of the 500 records there were but 177 (138 + 39) in which it was the lowest, and in 207 (10 + 10 + 59 + 105 + 12 + 11) either the second or the first was the

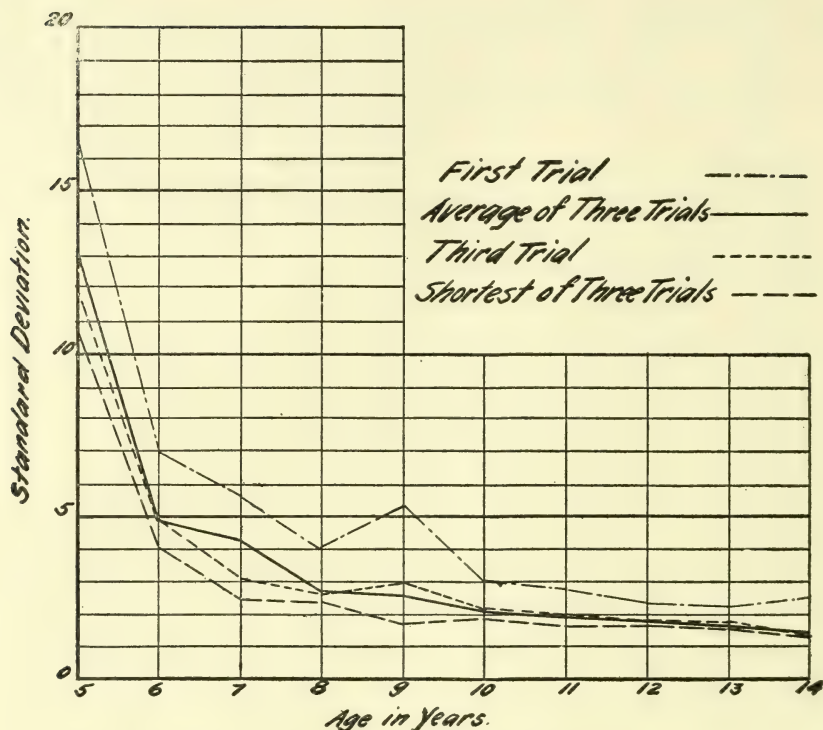


CHART II.—Standard deviations for the data of Chart I.

Relative lengths of the three trials.	Ages										Total
	5	6	7	8	9	10	11	12	13	14	
I = II = III						2		3	3	2	10
I = II < III				2		1	2	1	3	1	10
I = II > III		2	2	6	4	4	7	6	5	3	39
I > II = III	2	5	6	10	7	5	7	9	3	5	59
I > II < III	13	11	16	13	11	7	12	7	8	7	105
I > II > III	17	18	17	11	12	14	7	5	16	20	138
I < II = III	1	1	1		3	1		1	4		12
I < II < III	2	3	2		1			2		1	11
I < II > III	15	9	6	8	12	16	15	16	8	11	116
Total	50	50	50	50	50	50	50	50	50	50	500

TABLE XII.—Results for each age in terms of the relative lengths of the three trials. The =, &gt; and &lt; signs indicate the relative lengths of the time records of the three trials. The numbers in the body of the table show the number of individuals of each group whose three time records were in each of the possible combinations of relative lengths.

lowest. Although practice is probably the dominant factor in the variability of the length of time records, the presence of other important factors is shown by the many individual cases in which the first trial or the second trial was shorter than the third. Observation of children working at the test reveals the fact that over-hurrying, change of method of handling the blocks, or difficulty in getting some block down into its recess, may make the third trial longer than the others, and that there is an element of luck which makes the third trial index an unfair one in many cases. This standard is therefore undesirable.

Taking up the *shortest* of three trial index one finds in chart II that its curve is the lowest and is almost as smooth as any. The smoothness of the curve for the *average* of three trials is somewhat discounted because of its representing averages of averages, its position for each age being determined by the average of 150 time records while the corresponding positions of the other curves are determined by averages of 50 time records each. By the criterions of amount of variability and regularity of variability, the shortest of three trials is therefore the best of the four standards.

After this statistical study which led to the choosing of the shortest trial standard some time was spent in studying individual children, following the form board test with a thorough mental examination. In many cases the shortest trial index was found to be unsatisfactory and in some it was quite misleading. It was found that if the time records of the first trial and of the shortest trial were averaged, an index was obtained which usually agreed with the conclusions from mental examination of the child. Obviously, therefore, the first trial was of such importance that it could not be neglected. It was then decided to adopt tentatively the average of three trials as a standard. Applied to the records of the cases that had been examined and to several additional ones, it proved to be the most satisfactory of anything that had been tried. Without doubt, the average of three trials is a more reliable index to the mentality of a child than is any other single

numerical index, but even this is too mechanical and in many cases is misleading.

While this application of the average of three trials standard to individual cases was being made, another method suggested itself. It was tried on a number of cases and the preceding data were gone over from the new point of view so far as possible. It proved satisfactory and was welcomed because it included important features of the test which in the effort to reduce the records to the form of an index of one number, had been reluctantly excluded. The method is as follows. The child is introduced to the test with practically no instruction concerning it, merely the remark, "Let us see how quickly you can put the blocks into place". His first reactions are studied and full note is taken of his behavior and of his efforts until he succeeds in getting the blocks into place, or shows that he cannot do it. After this first trial, any instruction necessary is given to make him understand where the blocks belong and that he is to replace them as quickly as possible. Then the second and the third trials are given, starting him each time from the signals, "Ready—Go", urging him and giving him every chance to make the best possible records. The shortest of the two time records is taken as his time index. This with the notes taken on the first trial and the records of the handling of the blocks as taken by an assistant, constitutes the standard record. This method allows the use of Witmer's idea of carefully studying the first trial and at the same time it permits the use of the shortest trial time index which statistical checks had shown to be the most satisfactory. Most normal children proceed to place the blocks properly without instruction, and so make a fairly good time record on the first trial. An occasional child will fail to set the blocks entirely down into place or will even fail to lay them upon their proper recesses. These can easily be set right before the second trial. Of defective children, some require considerable help and several startings before they understand what is to be done. All of this is to be reported in the first trial notes. It was shown in table I that fourth and fifth trials usually differ little from the third trial, so even if con-

siderable practice is allowed in this so-called first trial, it will make little difference.

#### CORRELATION OF TIME RECORDS WITH AGE

This rambling search for a method of treating the data, and the consequent adoption of a new method of conducting the test came of course, after the data from the 500 selected children has been taken. All of the tests had been given in exactly the same way and under the strict requirements laid down at the beginning. While it is to be regretted that the data were not taken with an uninstructed first trial, as in the method finally adopted, it makes absolutely no difference in the following results and conclusions which are limited to variability of time records with age and sex. The averages may be slightly lower than they would have been by the new method, but excepting the coefficient of correlation, every result is shown clearly by the curve for each of the four standards that have been considered. The conclusions are drawn from the direction of the curves and not from their heights, so they apply to the new method whose curve (See chart IV, page 49) differs from that of the shortest of three trials much less than those of the other three standards do. With an uninstructed first trial the errors might not have been exactly the same, but for the purposes for which the records are here used the difference is negligible.

The general direction of the curves in chart I shows a negative correlation between age and the time required for placing the blocks. The coefficient of correlation for the shortest of three trials standard calculated by Pearson's products-moments method is 0.384. Considering the bold curve taken by the line of averages, this is not a low correlation. If the records for the five year old group are not included the coefficient is 0.465.

#### AGE VARIATIONS

In studying the age variations it is advantageous to consider simultaneously charts I, II, and III. The first two have been mentioned. Chart III shows the average numbers of errors made by children of each age. According to charts I and II many five

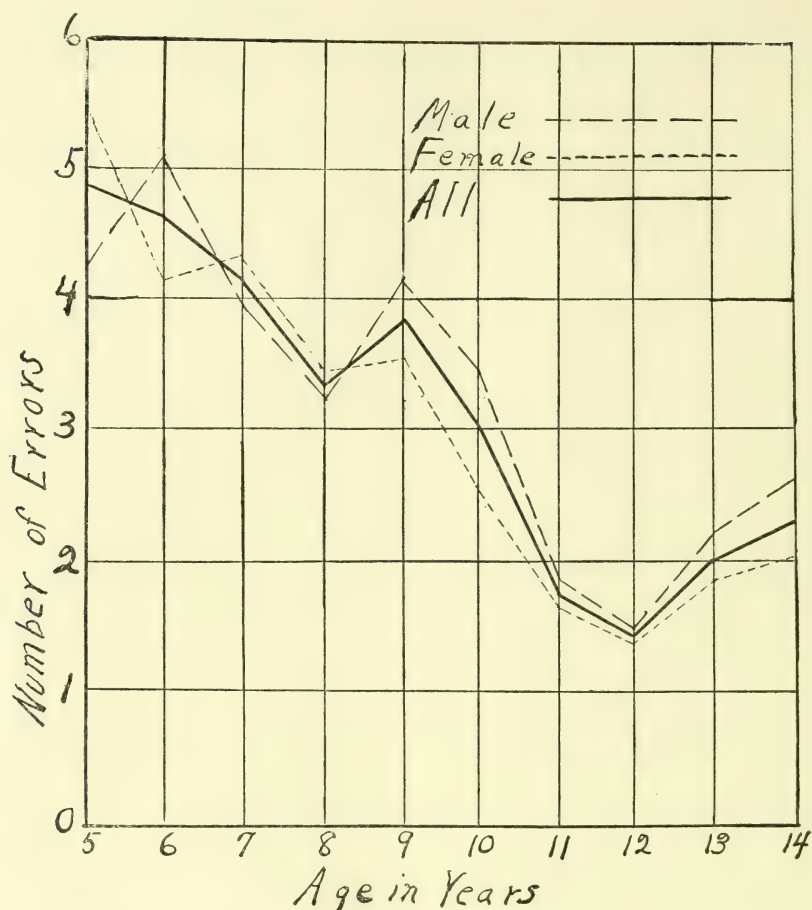


CHART III.—Average number of errors in three trials.

year old children require a long time for placing the blocks, both the average time and the variability being very high as compared with the other age groups, but chart III shows that in the number of errors the five year old group is comparatively not so high. These facts are easily understood by anyone who has watched five year olds working at the test. They move slowly in handling the blocks and cannot be made to hurry, working so deliberately that were their form perception as keen as is that of older children, they would make no errors. The number of errors made is therefore relatively low as compared with the curve of the time

records. A few of them hurry, this fact partly accounting for the wide range of the time records. The records are evenly distributed from these shorter ones to the extremely long ones, showing no mode or modes. Fatigue and a waning of interest are noticeable in some five year olds, but probably in no other age. A few six year olds work slowly and so avoid making so many errors. Except for these few children, six year olds are much more like seven year olds than they are like five year olds in this test. Considerable emphasis is sometimes laid on the effects of second dentition on seven year old children.<sup>3</sup> Some of the curves in these charts are a bit irregular at this age but on the whole the seven year olds seem to hold their own with other groups. Nine year olds fail to do this, their curves showing decided irregularities. There is no explanation at hand for these erratic tendencies, but it is a common observation by principals and teachers that nine year olds are the most puzzling children they have to deal with at school. Gilbert's curves show irregularities at this age.<sup>4</sup> Goddard<sup>5</sup> and Wallin<sup>6</sup> have found in separate investigations that the Binet tests for nine year olds are more uncertain than for any other age.

It seems that in form perception and motor ability, as they are required in this test, there is practically no gain after a point somewhere about the age of twelve. The better records made above that age are due to planning and to more determined effort. In these charts we find that at ages thirteen and fourteen the blocks were placed more quickly than at twelve, the standard deviations were lower, but the number of errors was greater. To those who have observed the testing of these older children it is evident that hurrying is the cause of the errors. In order to place the blocks in less than twelve or fourteen seconds, the child has to handle them so rapidly that there is not time enough to

<sup>3</sup> Chamberlain, *The Child*, p. 70 and ff. Gilbert, *Yale Psychological Laboratory Studies*, II, 93, and preceding tables and charts.

<sup>4</sup> *Ibid.*

<sup>5</sup> Reported in public addresses.

<sup>6</sup> Reported before American Psychological Association, Dec., 1911.

perceive the block's form and then to compare its image accurately with the recess forms. Eye movement, mental processes, and hand movements must go on simultaneously and very rapidly, and it is necessary to take a chance that the first impression of the shape of a form is correct. So these errors are not caused by inability to perceive form, but by a blurred and incomplete perception due to the rapidity of the work. Yet this gives the same result as inability to perceive form, for these older boys and girls confuse most often the same forms that the slow working young children do. Table XIV shows that for all ages the 0-4 error is by far the most frequent; for both five year olds and fourteen year olds about 12% of the errors being of this one kind.

The practical conclusion to be drawn from these studies of the age variations is that the time required for placing the blocks varies with age of children: that excepting five year olds, averages or modes of records for each age should be quite reliable as standards with which to compare individual records.

#### SEX DIFFERENCES

Sex differences are of no importance in the form board test. The average of time records for boys are slightly below that for girls, the two being 17.8 and 18.2 respectively. Boys, especially the older ones, enter into a "Ready—Go" test more energetically than do girls, but the extra errors that they make because of hurrying partially sets them back. So their average gain in time, as shown above, is only 0.4 seconds. This gain is not evenly distributed by ages. The fact that at ages eleven and twelve, girls make the better records and at thirteen and fourteen the boys excel is probably due to changes connected with puberty.<sup>7</sup> However that may be, there can be no doubt as to the effect of the greater hurrying so noticeable in older boys. It is clearly shown in the shortness of their time records at ages thirteen and fourteen in chart I, and in the large number of errors made at those ages as shown in chart III. Standard deviation charts for

<sup>7</sup> Burnham. *Ped. Sem.* I, 181; Bryan. *Am. Jour. of Psych.* V, 173; Gilbert, cited above.

the two sexes are not given because no sex differences are apparent in them.

## KINDS OF ERRORS

The number of times that each kind of error was made is shown in table XIII which gives for the 500 selected chil-

	Recesses										Total
	1	2	3	4	5	6	7	8	9	0	
Blocks											
1		1		41	22	1	1	32		4	102
2	7		4	18	9	4	7	5	4	32	90
3		5		2	2	5	7	3	40	15	79
4	17	2			28		3	8		44	102
5	16	3		24		2	6	24		4	79
6	4	2	2	7	5		38	51		16	125
7	10	10	14	11	29	30		46		40	190
8	76	5		29	65	9	12		1	7	204
9		5	34	2		4	2	2		9	58
0	12	30	3	255	37	1	15	32	6		391
Total	142	63	57	389	197	91	91	203	51	171	1491

TABLE XIII.—Distribution of the kinds of errors made by the 500 selected children. (For further explanation, see corresponding table for backward class children, page 20.)

dren data corresponding to that in table VI (page 20) for the backward class children. The most frequent error was that of attempting to put block 0 into recess 4. This error occurred 255 times. Block 0 was misplaced more frequently than any other, altogether 391 times out of the possible 1500. This is due partly to its form and to the diagonal position of its recess, and partly to the fact that the recess is often hidden by the right arm of the child. The only forms not once confused with each other were 3 and 1, 9 and 1, and 9 and 5. Block 9 was the most often placed correctly, its record being only 58 misplacings out of the possible 1500. From these results it is not worth while to attempt to draw conclusions as to the relative complexity of the forms, their resemblances, and the effects of the different positions of the recesses. These matters if worth investigating, would demand an elaborate study based on facts of form perception and visual illusions which have not yet been worked out.

One important conclusion is to be drawn from the data as

arranged in table XIV, analogous to table VII (page 21). Here the twelve most frequent errors as shown in table XIII, are arranged according to ages of children. In the upper horizontal line are the frequencies of each of these twelve errors made by five year olds and in the other horizontal lines they are shown

	Kinds of errors											
	0-4	8-1	8-5	6-8	7-8	4-0	1-4	3-9	7-0	6-7	0-5	9-3
Ages 5	12	3	6	2	3	3	2	2	2	3	2	3
6	10	3	3	2	1	4	2	5	3	4	2	4
7	20	2	5	2		4	4	2	1	1	1	4
8	21	6	5	5	5		1	2	2	5	2	1
9	17	5	2	4	3	3	2	2	3	1	3	2
10	14	7	4	6	4	2	1	1	2	2	3	1
11	16	11	4	3	3	3	3	2	3	1	3	
12	21	5	3	2	5	3	2	2	5	2	1	
13	15	4	1	3	4	1	5	6	1	1	2	1
14	13	4	3	2	1	2	3	5	3	2	2	1
Average	16	5	4	3	3	2	2	2	2	2	2	2

TABLE XIV.—The twelve most frequent kinds of errors of the 500 selected children arranged according to ages. (Compare with table VII, page 21.)

for children of other ages. The most important fact revealed is that certain kinds of errors are favored by children of all ages. The occurrence of the 0-4 error varies little with age and the others are evenly distributed. As has already been stated the errors by older children are due chiefly to hurrying. They can discriminate these forms with certainty when not hurried, but if they get only a glimpse of the block form and have little time for imaging it and comparing it with the recess forms, they make the same errors as the younger children. The blurred perception of the older children and the faulty perception of the younger ones give the same results.

It has been stated by some who use the form board test that if a child persists in making the same kind of errors he is lacking in learning ability. This seems plausible on the assumption that he ought to recognize the situation and not repeat the same errors. But it is not borne out by results. On the contrary it is shown that bright children as well as dull ones often persist in the same

kind of errors and that most of the extremely backward show no tendency to do so. The following record of a bright nine year old boy is a conspicuous case of repeating particular errors. In this case the two errors 2-8 and 4-0 were each repeated and they might have appeared in all three trials had not the order in which the blocks were piled made it impossible. The number of normal children out of the 50 of each age who repeated one or more errors was as follows:

	FIRST TRIAL	SECOND TRIAL	THIRD TRIAL
	3	5	7
	1	282	5
	5	7	8
	8	1	2
	6	404	404
	2802	0	6
	9	9	3
	0	3	1
	7	8	0
	4	6	9
	—	—	—
	26 sec.	23 sec.	19 sec.

Age	5	6	7	8	9	10	11	12	13	14
Number who repeated errors	14	14	12	8	14	7	9	11	4	7

There is a type of defectives who persists to an extreme degree in repeating errors or in trying to put every block picked up into a certain recess. The records of the 605 backward class children show a dozen such individuals, but the great mass of the backward class children repeated errors less often than did the normal children.<sup>8</sup>

### SUMMARY

This study of the 500 selected children may be summarized as follows:

Children vary so widely in their development and advancement that in order to reveal their changes in any capacity from year to year, a large mass of data would be necessary. The collecting of this is impracticable for a test requiring the time that the form board does. The difficulty was partly handled by collecting a reasonably large amount of data from carefully selected homogeneous groups.

<sup>8</sup>No exact comparison is possible. 161 of the 605, as compared with 99 of the selected 500 normal children repeated errors, but since their total number of errors of all kinds was more than twice the number made by the normal children the chances for repeating were far more than enough to make up for the larger number who did repeat. Also there were 105 more individuals in the group.

Four possible indices for representing a child's form board ability were considered; the time records for the first trial, for the third trial, for the shortest of three trials, and for the average of three trials. The first trial index was eliminated because of its wide and irregular variability. The third trial index proved to be unreliable because bright children often fall back badly on the third trial through over-hurrying, change of method of handling the blocks, or bad luck in fitting them into the recesses. The shortest of three trials index has the lowest variability of the four and is almost as regular by ages as any other, so from the statistical point of view it is the most reliable. When applied to individual children it failed in many cases to agree with the results from careful mental examinations. The average of three trials index was next tried. It proved more satisfactory in its application to individual cases, evidently because it gives weight to the first trial, a feature not embraced by the variability criterion. The outcome was a modified method of giving the test and of treating the data. Since this new method involved a change in the testing procedure it could not be applied in every way to the data from the 500 selected children. Fortunately the important results from which the data had been taken are the same, no matter what standard index is used. The following conclusions therefore apply to the new method and to all others in which the time element is made the main feature.

1. There is a negative correlation between age and the time required for placing the blocks.

2. Five year olds show an extremely wide individual variability and on the average their time records are comparatively long. Because nearly all work so slowly, their number of errors is lower than would be expected, judging from the number made by other age groups. A few six year olds work slowly like five year olds, but the differences between ages five and six are much greater than the differences between ages six and seven. There are unexplained irregularities in the records of nine year olds. After the age of twelve there is practically no gain in form board ability except that due to better planning and to greater

effort. As a result thirteen year olds and fourteen year olds make shorter time records but the extra hurrying causes them to make more errors than the twelve year olds.

3. Excepting that for five year olds, averages of time records for each age should be quite reliable as standards with which to compare individuals.

4. Sex differences in the form board test are negligible.

5. The o-4 error is much the most frequent but there are others that are favored. Block o is the most often misplaced and block 9 the least often. These facts are equally true for children of all ages.

6. A tendency to repeat certain kinds of errors is not indicative of weak mentality unless persisted in to an extreme degree.

## V

### A STANDARDIZATION OF THE FORM BOARD TEST

There is a general tendency at present to over emphasize mental tests. Many of the uninitiated expect tests of mentality to be as decisive and as reliable as the acid test, and some experienced examiners are quite dependent on them. They fail to realize fully that mind is a *function*, and that it is the resultant of a complex of factors which no one test can even approximately measure. Co-ordinated systems of tests such as the Binet-Simon cover a number of the factors, but no team of tests has as yet been offered which comes near covering all. The investigator believes that for a single test the form board is unexcelled; that an examiner who is cognizant of the limitations of tests and who knows how to articulate their results with his judgment based on general observation of the child, will find in this test a most valuable and reliable aid. It is from this viewpoint that the following standardization is given. In the preceding studies conclusions were reached on most of the important features of the form board test and a satisfactory method of using it and of interpreting its results were worked out. The various features of this method have been described only in connection with the studies through which they were evolved, so at the beginning the following complete statement is necessary.

#### THE METHOD OF APPLYING THE FORM BOARD TEST

The form board lies horizontally on a table, its lower edge even with the edge of the table next to which the child stands. The table must be low enough to allow him to lean well over the board and to look down upon its center. The blocks are placed in three piles on the table next to the upper edge of the board, no block in the pile nearest its recess, the lozenge and the elongated hexagon not in the same layer, and the star in the lower layer. This is the arrangement at the beginning of each of

three trials. The child is introduced to the test with no instruction concerning it except, "Let us see how quickly you can put the blocks into place." His first reactions and his behavior until he succeeds in getting the blocks into place or fails are carefully studied. After this first trial he is given any instruction necessary to make him understand where the blocks belong and that he is to replace them as quickly as possible. Then he is given a second and a third trial, in which he is encouraged and urged in every way to make the best record of which he is capable. These last two trials are timed with a stop watch and the shortest of the two records is taken as the child's form board index. In addition the examiner records an estimate of the child's co-ordination and poise;<sup>1</sup> of whether he plans ahead; of whether he successfully uses both hands at the same time; and after the test is completed he dictates to his assistant his observations of individual features. During the testing the assistant has taken a complete record of the order in which the blocks were handled.<sup>2</sup>

The record of the test then consists of four parts;—(1) An account of the first trial. (2) The shorter of the two time records. (3) A record of co-ordination, poise, planning ahead, use of the hands, and general observations. (4) A record of the handling of the blocks. This is not so cumbersome as it looks. All of it is taken while the child is at work, except part of the account of the first trial and the general notes, and these unless the case is an unusual one are stated in a few words.

#### THE STANDARDIZATION

This work was undertaken through the testing of another group of public school children. The results from the 500 selected children reported in the previous section could not be used because in those tests careful instruction was given before the first trial instead of before the second trial as in the method finally adopted. Also, in order to make the group as nearly homogeneous as possible, they had been selected according to

<sup>1</sup> For meaning of these qualities as used here see p. 19.

<sup>2</sup> For method see p. 14.

requirements which made them above the average of ordinary children. Therefore new data had to be collected. It was proposed to test 250 of each of the ten ages, 2500 in all, but this was found to be too large an undertaking. Inasmuch as the five, thirteen, and fourteen year standards would be of less value than the others,<sup>3</sup> it was decided to include fewer of these ages and to spend the available time on the ages for which the standardization would be of the most value. In all 1537 children were tested. Except that no especially backward or peculiar children were included there was no selecting. The results arranged according to the four parts of the records are as follows:

(1) No attempt was made to standardize the features that are to be observed in the first of the three trials. The examiner is not limited as to what he shall look for in this part of the test. He must have his whole stock of psychological knowledge open for apperceiving whatever is brought out, so the features that strike him as important may vary widely in different children.

(2) The age distribution of the time records is shown in table XV. Corresponding closely to this is table X, page 29. The latter displays a much more restricted distribution because the 500 children were selected with the purpose of securing homogeneity, but the age variability of the time records is much the same in the two tables. Averages of the time records for each age, and their limiting zones are given in table XVI<sup>4</sup> and

<sup>3</sup> See pages 8, 36, and 38 for reasons.

<sup>4</sup> In the following table these time averages are compared with those which Goddard obtained by a somewhat different method from 250 children. (Training School, IX, 51.)

AGE	GODDARD'S AVERAGES	AVERAGES FROM THE PRESENT INVESTIGATION
5	29.5	37.6
6	27.5	26.5
7	24.5	23.3
8	21.8	20.6
9	19.3	18.6
10	18.2	16.7
11	17.6	14.9
12	15.9	13.8

Time in seconds.	5	6	7	8	9	10	11	12	13	14
75	1									
58	1									
57										
56	3									
55	1									
54										
53	1									
52										
51										
50	2									
49	1									
48	1									
47	1									
46	1									
45	2									
44	2									
43	4									
42	5									
41	2	2								
40	2	1								
39	3	1								
38	6	1	1							
37	2	3								
36	1	1	1							
35	6	3								
34	5	6	1		1					
33	3	3	3							
32	3	9		2						
31		11	2	1	3					
30	2	10	4	3	1					
29	4	5	6	2	3					
28	3	11	5	5	2					
27	1	9	13	2	2	1				
26	5	11	16	6	4	2				
25	1	14	7	6	4					
24	2	21	17	4	3	6	1			
23	2	6	13	12	6	4		1		
22	1	7	18	18	10	2	2			
21		12	24	28	8	9	1	1		
20		11	13	39	19	11	2	2		
19		6	8	19	19	22	4	1		
18		6	10	13	31	16	7	2		
17			5	20	29	28	17	9	2	1
16			5	16	26	28	26	8	5	2
15			1	8	30	38	39	22	6	4
14				2	9	24	29	31	11	6
13					4	17	19	20	11	9
12						13	17	21	23	16
11							4	17	15	16
10							2	6	3	18
9							2		4	8

TABLE XV.—Distribution of the time records of the 1537 normal children.

Ages	Number of cases	Average time	Zone limits	Standard deviation
5	80	37.6	22-75	9.66
6	170	26.5	18-44	5.23
7	173	23.3	15-38	4.14
8	206	20.5	14-32	3.59
9	214	18.7	13-34	3.88
10	221	16.7	12-27	3.06
11	172	14.9	9-24	2.32
12	141	13.8	10-22	2.29
13	80	12.6	9-17	1.85
14	80	11.6	9-17	1.85

TABLE XVI.—Time records of the 1537 normal children.

in chart IV. In the chart the heavy line represents averages of time records and the shaded portion includes the 1537 records. For example the average time required for eight year olds to place the blocks is shown to be 20.6 seconds, while the shortest and the longest records for that age are 14 seconds and 32 seconds respectively. Table XV shows that in most cases the records are well enough distributed over the zones to make the zone widths a rough expression of the variability at each age. In chart IV we have a complete standardization of the time features of the form board test. By referring to it one can quickly interpret the time record of a child. Unless his record falls outside the zone limits for his age he is to be considered normal in this important feature of the test, but of course the nearer it comes to the line of averages the better. It would be presuming too much to claim that these zones definitely divide the normal from the sub-normal, but in order that the line of averages may be of the most value the zone width at the different ages must be considered with it. For instance it is shown that a seven year old child's record may be considerably farther above the line of averages than could be allowed in a ten year old's record without suspecting sub-normality. Attempts to produce a better chart than this by displaying the standard deviations instead of the distribution limits have failed. It is possible in that way to give more regular zone boundaries than are seen in this chart, but there is no basis for reducing them to a scale that would satis-

factorily represent standard deviations in connection with the line of averages. In fact the chart as given comes remarkably

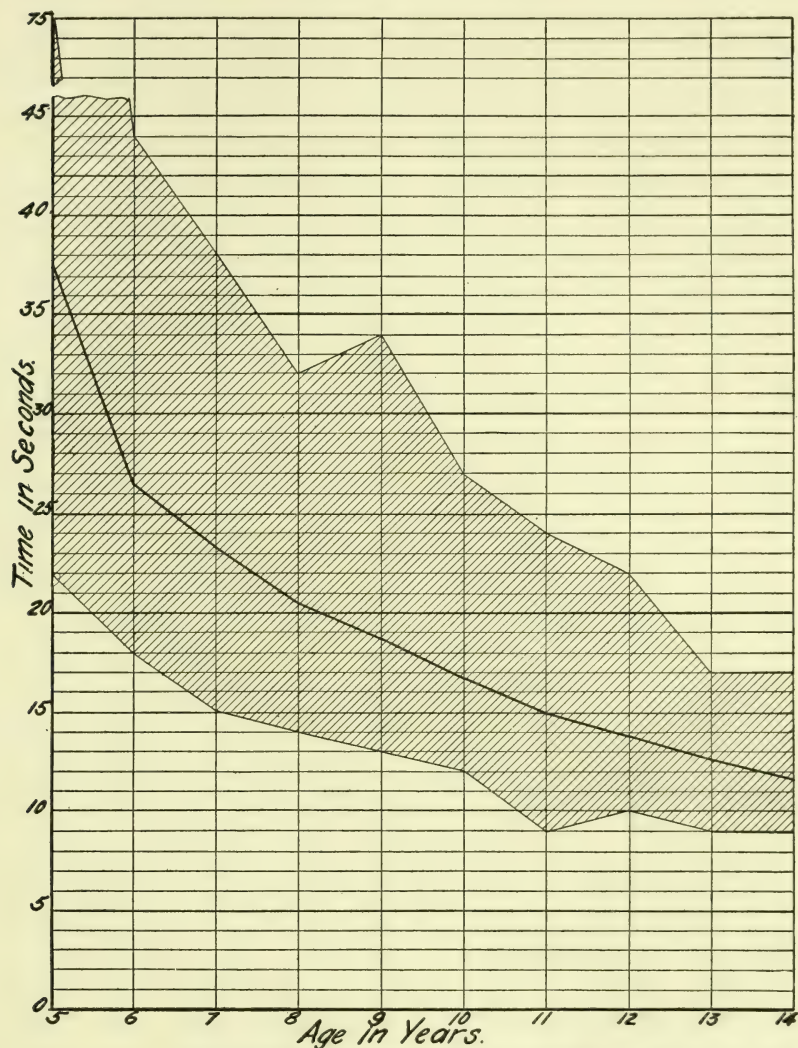


CHART IV.—THE FORM BOARD TIME CHART. The heavy middle line shows the average time record for each age, and the shaded zone is limited by the shortest and the longest records for each age.

near to presenting the standard deviations. It happens that by multiplying the standard deviations by 5 they can be compared

with the zone widths. This is shown in the accompanying table:

Age	5	6	7	8	9	10	11	12	13	14
Width of time zone	53	26	23	18	19	15	13	12	8	8
5 S. D.	49	26	24	19	18	16	12	12	10	10

This approximation of an index of variability by the zone widths adds greatly to their reliability. The two or three serious irregularities in the zone boundaries are objectionable, but they have a value in constantly reminding the user of the chart that records falling near the zone limits are of uncertain interpretation.

(3) To establish standards on poise and co-ordination it would be necessary to grade a large number of children who are defective in these features. Nothing in that direction was attempted in this study. None of these 1537 normal children were seriously lacking in either. Of the 605 backward class children, every individual who was given a low grading in one of these features proved to have other defects and to be mentally sub-normal. Hence the conclusion is to be drawn that serious lack in co-ordination or poise indicates mental deficiency.

Data were taken as to how many of the 1537 children planned ahead. The results stated in per cent. of each age group follow:

Age	5	6	7	8	9	10	11	12	13	14
Per cent. planning ahead	0	0	0	4	9	10	16	26	50	54

Obviously a child should be given considerable credit for planning ahead since very few younger normal children and only 8 per cent of the backward children do so,<sup>5</sup> while about half of the older normal ones do.

A record was also taken of the number who used the two hands at the same time successfully. The following statement is in per cent. of each age group:

Age	5	6	7	8	9	10	11	12	13	14
Per cent. using both hands	0	8	14	38	45	54	64	88	100	98

Here again the older children succeed best, but ability to use both hands at the same time successfully is not confined to children of quite so high advancement as is planning ahead. Nearly all of

<sup>5</sup> Table V, page 17.



is of value. Some children of all ages make no errors and the majority make less than four in the three trials at the test. A few of each age make as high as ten, an occasional one younger than ten years makes as many as twenty, while a few five year olds make more than twenty.

It has been worked out in previous studies<sup>8</sup> that attempts to place the lozenge block in the elongated hexagon recess is by far the most common error, and that certain other kinds of errors, especially those involving the flattened oval, are made quite frequently. Extremely backward children find all forms equally difficult, making as many errors of one kind as of another. So a tendency to favor these common errors is creditable to the child, many bright children repeating from one to three errors in the three trials,<sup>9</sup> but an extreme tendency to repeat an error, especially attempts to fit every block into some one recess, indicates quite low mentality.

This completes the standardization of the form board test for children between five and fourteen years of age. Of the four parts of the record, the time index is the most important because it is convenient for use in speaking of a child's form board ability and because it usually includes what is shown in the others. By this is meant that if a child makes many errors or lacks poise or is lacking in any other feature, his time record will be accordingly longer. The occasional cases in which a time index alone is misleading make it necessary to record the other features, and since this can be easily done it is best to make a full record in every case.

<sup>8</sup> Pages 21 and 40.

<sup>9</sup> Page 41.

## APPENDIX

### CHILDREN UNDER FIVE YEARS OF AGE

A group of thirty-five four year old children were given the form board test, the regular method being used except that the child was handed each block and in case he spent considerable time trying to fit it into a wrong recess he was told to try another. All normal four year olds can place the blocks if given that much help. The shortest time record was 20 seconds, the longest 91 seconds, and the average 46 seconds. Three of the thirty-five made no errors, one made 42, and the average number made was 11. Seventeen made their best record on the second trial and eighteen on their third. Because they were handed the blocks and were not allowed to spend too much time trying a wrong recess, the effects of fatigue are not so noticeable in the time records, but the majority showed waning of interest and fatigue on the last trial.

Nine children between three and three and a half years of age were tested in the same way except that they were given but two trials. Their shortest time record was 49 seconds, the longest 113 seconds, and the average 69 seconds. The number of errors varied between 12 and 24, the average being 16. Six of the nine did better on the second trial than on the first.

Seven children between the ages of two years three months and two years six months, with considerable help gave time records ranging from 52 seconds to 148 seconds and an average of 92 seconds. Their errors ranged between 4 and 25 for the two trials, with an average of 17. Four did much better on the first trial than on the second. All of these children perceived the relation of block form to recess form for at least the circle and the square. They commonly confused the cross with the star, the oval with the semi-circle and the circle, and the triangle, the lozenge, and the elongated hexagon with each other. If they happened to get the lozenge crosswise over its recess, they usually would not turn it without help. They often searched in the piles for a block for some particular recess or picked up the circle in preference to others. Some tired of the test after a trial or two but two cried because they were not allowed to continue.

The test was tried on several children between one and a half

and two years of age. The form board was laid on the floor. With much help one child placed six blocks and others placed two or three. Some showed unmistakably that they perceived the circle form and certain of the other more simple ones. The majority piled the blocks one upon another instead of attempting to fit them into recesses. At the Philadelphia Infants' Home, a form board was left in one of the rooms where a dozen of these little tots spent most of the day, and their nurse attempted for a week to teach them to put the blocks into place. Some made a little progress but all continued to pile them and not one learned to complete the test.

#### ADULTS

Adults place the blocks a little more quickly than do fourteen year olds. Most of their records fall between 9 and 12 seconds. An occasional 8 second record is made, and three individuals out of more than a hundred made records of 7 seconds in one of their first three trials. Practically all adults plan ahead. The most successful handling of the blocks is a rhythmic alternating of the two hands, one hand fitting a block while the other is picking one from the piles. When one attempts to fit two blocks into their recesses simultaneously time is lost, probably because of the attempt to divide the attention.

#### CHILDREN OF LOW MENTALITY

There is no kind of reaction to the form board test that is strictly typical of any one grade or class of defectives. This is partly due to the fact that each of our standard classifications has its own basis, such as industrial capacity, linguistic ability and educability. Accordingly children may rank quite differently under different classification systems, and the form board test could not be expected to label individuals directly for their place in a mental scale unless such scale had form board ability as its basis.<sup>1</sup> For diagnostic purposes it is therefore necessary first to compare the individual's form board reaction with the reaction of normal children, and then after he has thus been approximately placed, to study his reaction in comparison with that of other defectives. Hence the importance of normal standards.

<sup>1</sup>Form board time records do not correlate well with Binet Test results, children who are considerably retarded according to the Binet scale usually being more successful at the form board test than are normal children of the corresponding Binet age.

All kinds of mental defectives who can do anything with the form board were included among the 605 backward class children whose tests are reported in Section III of this monograph. But since that study was made before the standards for normal children were established, it is worth while to supplement it with the following notes on tests of defectives made after the work on normal children had been completed.

Seventy-six imbeciles and idiots ranging in age from nine to seventeen were given the form board test,—some in the Psychological Clinic of the University of Pennsylvania, some in the Pennsylvania Training School for Feeble Minded Children at Elwyn, and some in small private schools. As to the time records, the records of errors, and the records of other items that are included in the standards given in the last section of this monograph, these later observations of defectives seem wherever possible to corroborate the conclusions drawn there. They show nothing that disagrees with those conclusions. Of the seventy-six defectives, forty-two succeeded in putting the blocks into place three times, fourteen placed them once but not three times, and twenty failed to place all of them even once. Of those who placed them one or more times, thirty-three required more than 30 seconds for the shortest trial. There were several times as many errors as would have been made by normal children, and there was only an irregular tendency to favor the 0-4 error. Very few attempted to use both hands at the same time and but nine did so successfully. None planned ahead. A large number were lacking in poise; some being confused by their own efforts as well as by the urging and assistance offered by the examiner. In some cases the confusion was only temporary, poise being regained and the work proceeding successfully for a time, but in others even after a promising beginning, control was lost and the efforts ended in utter confusion. Some of these defectives are at an opposite extreme from those who lack poise, being abnormally inert and stolid. They work at the form board in a listless, indifferent manner, lacking either the inclination or the ability to start quickly and to work rapidly. The most of these make somewhat better records when urged strenuously. A normal child is alert but at the same time has self-control and poise.

There is no testing device that makes a stronger appeal to the interest of children, both normal and defective, than does the form board test. It is therefore a good test of attention. Practically every child gives it the best attention of which he is capable. Twenty-four of the seventy-six defectives gave the test

undivided attention as long as the examiner wished them to work at it, although some of them worked slowly and made many errors. Fourteen gave good attention through one trial but wandered from the task before told to stop. Thirty-one showed various degrees of flightiness, some attending to the test but a few seconds at a time, and others almost completing a trial. Some of these returned to it of their own accord, others had to be reminded by the examiner. Three of them refused to return to it. Seven could not be interested in the test at all, and made no effort to place blocks. Fatigue is a factor in the case of many who lose interest.

The emotional reaction of defectives to the form board test is extremely interesting. Affectively, only ten of these seventy-six reacted like normal children. Seventeen were apathetic, the test arousing little or no interest in them. Thirty-three found great enjoyment in it, working enthusiastically, some talking and chattering while at work and many of them expressing extreme joy when a block or blocks were placed successfully. It was probably the most difficult piece of work that some of them had ever done, hence their feeling of triumph and satisfaction in succeeding. Some of the more excitable ones would of course react in the same way to any test involving activity. The other sixteen gave various kinds of curious and inconsistent reactions. One large boy started well but before half of the blocks were placed he began to weep hysterically and ran away refusing even to look backward. Several others wept and wailed, attracted to the test but forced to leave it because of embarrassment and excitement.

These notes give but a glimpse of what can be observed in form board tests of defectives. For instance the attempt to group the seventy-six cases on the basis of attention might be extended to include an analysis of each individual's volitional complex. It would cover not only his power of attention, but also his initiative, his self control, and the intensity of his effort. A full report would include the painting of a clinical picture of each case. How much of this is profitable depends on the individual case and on the extent to which other tests and means of analysis are employed. These notes are suggestive of what may be worked out from the form board test, and they emphasize the fact that *normal* standards must be the basis upon which each defective's reaction to the form board test is to be interpreted.

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## The Influence of Stimulus Duration on Reaction Time

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## I. INTRODUCTION

In 1820 Bessel established beyond doubt the fact of the "personal equation", the existence of which had been indicated twenty-five years earlier by the historic disagreement between Maskelyne and his assistant over the timing of the transit of a star.<sup>47</sup> Later, the chronograph was introduced and took the place of the eye and ear method used by Maskelyne and Bessel. The use of the chronograph reduced the "personal equation" factor, but did not eliminate it. The "personal equation" involves, *inter alia*, the time elapsing between an expected stimulation and a muscular reaction to that stimulation. Since that time the reaction movement and its consciousness have been examined from almost every possible angle. The conditions of experiment have been carefully arranged and varied; in particular, the quality and intensity of the stimulus have been carefully controlled and the effect on reaction time of changes in these factors noted, and the influence of the general physiological condition of the reactor has been studied.

It would seem that of all fields open to psychological study that of reaction time offered least promise of yielding new fruits to labor. The complexity of the conditions governing the reaction movement and the stimulus would, of course, allow of endless permutations and combinations of these two factors. But unless the rearranging of conditions were done with some particular problem in view it would be less than worth while to do so.

The present investigation did not start with the idea of merely making a new combination of the conditions of reacting, but starting with a problem somewhat different from that which was finally worked out, was driven to the latter, because it is an essential step in the solution of the former problem and it had received very little attention.

The original intention was to investigate the characteristic difference between auditory and visual reaction times, and to

ascertain the conditions under which this difference obtained. It was suggested that as the physiological effect of the stimulus on the retina was probably of longer duration than the effect of sound waves on the hearing mechanism, some light might be thrown on the characteristic difference in the reaction times to stimuli of different modes if the effects of varying the duration of the stimulus were known. For some reason or other little or no attention has been paid in reaction time experiments to the part played by the duration of the stimulus. Most of the authors neglect this factor altogether; one or two assume<sup>59</sup> that the effect of increasing the duration of the stimulus is the same as the effect of increasing the intensity, and shortens the reaction time. That this is the effect of variations in intensity of the stimulus has probably been established.

The only one who has made any attempt to vary experimentally the duration of the stimulus is Sven Froeberg.<sup>32</sup>

Froeberg undertook the problem of determining the influence of variation of the extensive, intensive and temporal magnitude of stimulus on the time of reaction. In the work on duration he used only visual stimuli, but in the intensity work he used auditory stimuli also. The stimulus used in the visual work was daylight reflected from paper of varying degrees of brightness. His stimulation apparatus consisted essentially of a large iron wheel 92 cm. in diameter with a rim 17 cm. wide. This wheel was driven by an electric motor at a uniform speed of one revolution in three seconds. The author does not specify how this uniformity was obtained except to say that the speed of the motor was regulated by a resistance coil. It is so extremely difficult a task to obtain anything approaching perfect regularity of revolution by resistance control of a motor that it may perhaps be doubted, in the absence of evidence as to the speed of the wheel, whether it did more than approximate the speed mentioned. The stimulus paper was mounted on a rim, slightly projecting, so that its rate of movement was one hundred centimeters per second or one millimetre per sigma. Between the wheel and the reactor was a screen having an aperture of 16 x 16 cm. into

which smaller paper screens having apertures of from 3 to 48 mm. square were inserted. Except in two cases the back-ground was black. The stimulus paper, carried by the revolving wheel, moved past this aperture. The raising of a drop screen, which covered the aperture, about  $1\frac{1}{2}$  seconds before the exposure, furnished a warning signal. Reaction was made by means of an ordinary telegraph key, and the Hipp chronoscope was used for recording.

The experiments on intensity will be mentioned later. In the experiments on duration five strips of white baryta paper 48 mm., 24 mm., 12mm., 6 mm., and 3 mm. wide respectively were used for stimuli. Inasmuch as the speed at which these papers traveled was one mm. per sigma their exposure times were 48, 24, 12, 6 and 3  $\sigma$ .

The intensity and the maximal size of stimulus were kept constant. The intensity was nominated 100, the size of the exposure was 3 mm. square. The average length of reactions by two reactors to each of Froeberg's five durations is given in the following table in sigma ( $\sigma$ ). R. T. signifies reaction time and M. V. mean variation. Each value is the average of 400 reactions:

	48		24		12		6		3	
	RT	MV	RT	MV	RT	MV	RT	MV	RT	MV
Subj. R.	191.1	11.3	193.5	11.3	196.4	10.9	198.7	11.	200.6	12.1
Subj. W.	173.4	7.9	175.2	8.7	177.4	8.5	179.2	9.3	180.7	8.8

Froeberg's conclusions are to the effect that reaction time increases by approximately equal arithmetical increments as the duration of the stimulus decreases geometrically. This is held to be true only over a limited range, when the threshold is approached the increase becomes more rapid. The differences found by Froeberg are slight, and their interpretation not altogether unambiguous. It seemed imperative that the influence of duration should be thoroughly investigated anew.

My work on the problem of the influence of duration of stimulus on reaction time was done in the Psychological Laboratory of the Johns Hopkins University, under the direction of Dr. Dunlap, in the years 1910-1912. The problem was thoroughly

worked out for auditory and visual stimuli. Especial pains were taken to have accuracy of all measurements, to have the conditions uniform and easy for the reactors, and to have an adequate number and distribution of reactions. In the visual work 37,050 reactions were recorded, and in the auditory work about 12,000.

### EXPLANATION OF PLATES

Plates I and X require no explanation beyond what is given in the text in describing the apparatus. In each case the broken lines represent reaction circuits, and the continuous lines represent the stimulus circuits.

The arrangement of the graphs is very simple. The abscissæ are indicated only once on a plate, but are the same for all graphs on a plate. They can be determined for any particular graph by means of a straight-edge from the graph in which they are indicated. They represent five sigma to a division, and every fifth division is denominated. The ordinates are not indicated, but are so chosen that the height from the base line of one graph to that of the graph above represents just 20% of the total number of reactions to that stimulus. By virtue of this arrangement all graphs are directly comparable, regardless of differences in the actual number of reactions made by the subjects.

The letter at the upper right hand corner of the plate represents the reactor, and the letter at the left hand side of each graph represents the stimulus. The small figures inserted at the right or left side of some of the graphs indicate the percentage of reactions the time of which would fall outside the graph at that end. For instance, in Plate II in the cases of both stimuli S5 and S4 there is .5 of 1% of the reactions below 70 sigma in length, and in the case of S2 there is .5 of 1% of the reactions above 220 sigma.

## II. REACTIONS TO AUDITORY STIMULI OF VARYING DURATION

The arrangement of apparatus used in this part of the investigation is schematically represented in Plate I.

The stimulus used was a noise produced in a telephone receiver held to the ear of the subject by the usual operator's head piece. The phone was placed in parallel with an ordinary electric buzzer which was in the experimenter's room. The current operating this buzzer was kept constant at .17 amperes by means of a rheostat Rh 2 in parallel with it. Another rheostat Rh 1 was inserted in series with the whole stimulus circuit. This rheostat Rh 1 affected the amperage on the whole circuit, while rheostat Rh 2 primarily affected the current on the buzzer. This rheostat Rh 2 was fixed at a certain position where it produced what was judged to be a satisfactory tone in the receiver, and was carefully fastened so that it could not be moved from that position during the whole course of the experiment. This buzzer, of course, could not be heard by the subject except through the telephone. The mechanism for producing the stimulus did not start this buzzer, but merely completed and then broke the telephone circuit arranged in parallel with it. The arrangement of apparatus necessary to produce this effect will be described a little later. The buzzer was kept in operation during the whole course of the experiment. This is an important detail inasmuch as it requires a few sigma to get the buzzer in full operation. The duration of the stimulus was controlled by a Wundt Fall Hammer (large model, made by Zimmerman). The two rear keys set at different heights were used for this purpose.

The two sliding keys of the fall hammer were inserted in the circuit in such a way that the hammer in falling completed the circuit through the make-and-break key BCI (See Plate I) and a moment later broke it again by means of the simple break

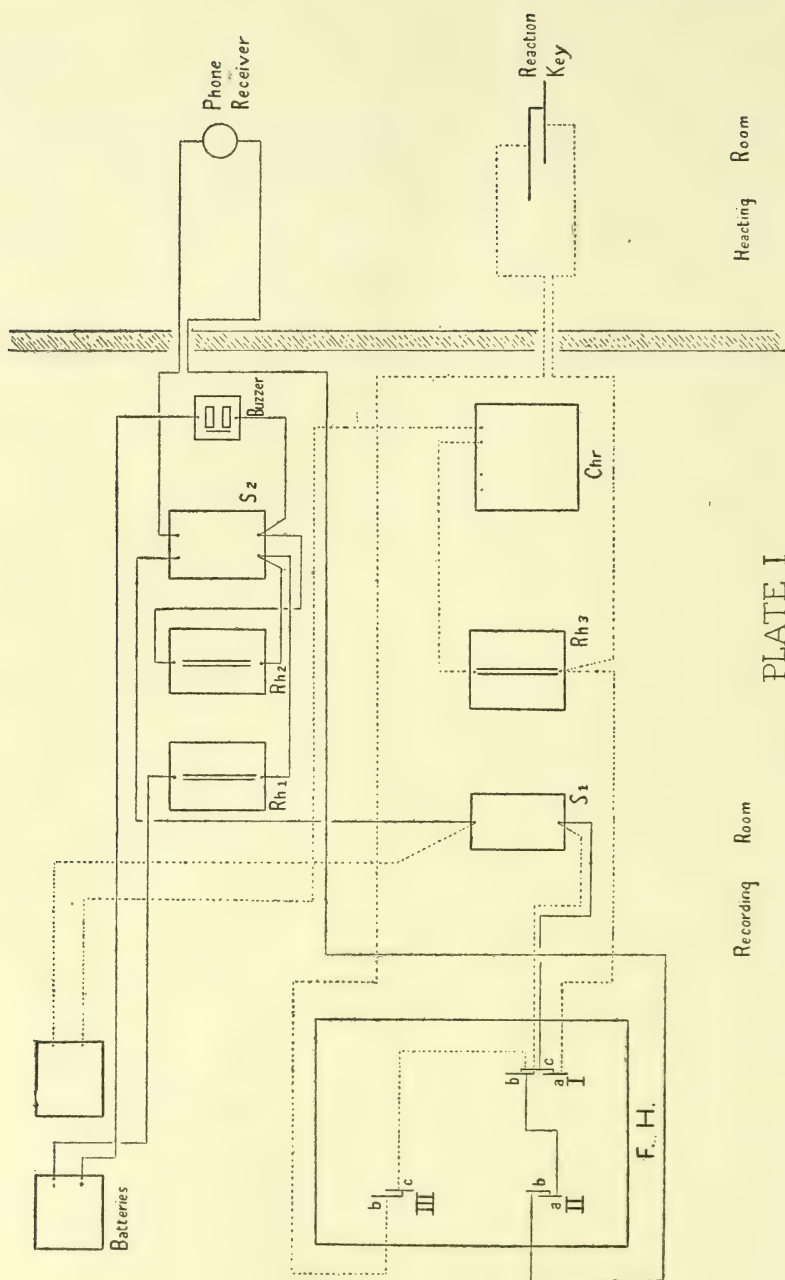


PLATE I

key BC II. The completion of the circuit in this way cut in the telephone so that the sound made by the buzzer, which was already in operation, could be heard by the subject. The breaking of the circuit cut the telephone out. Thus it can be seen that the length of time during which the buzzer could be heard was governed by the distance in height between the two rear keys. The counterpoise of the fall hammer was kept in a constant position throughout the experiment. Five positions for the two keys were chosen which gave intervals which seemed suitable for the purpose required. The durations of stimulus given by these five positions were measured by the spark chronograph method (two break sparks being used). The error of setting of the keys for any one of these durations was less than one sigma.

The recording instrument was a Hipp chronoscope, which was run from break to make. A constant current of .1 ampere was used on the chronoscope, measured daily and controlled by a rheostat, Rh 3.

The chronoscope circuit had two branches. In one was inserted the break contact of the upper moveable key of the fall hammer, and in the other the reactor's key and the make contact of one of the fixed fall hammer keys, BC III, which was kept closed, and hence out of function during reaction series. The chronoscope circuit was therefore broken at the instant of completing the stimulus circuit, and remade either by the reactor's key or by the lower fall hammer key. For control purposes the upper moveable key was set at a certain point, the reacting key was closed, thus completing the whole circuit, and a series of ten readings taken of the time of the drop from Key I to Key III. The variation of the chronoscope for a particular day having been ascertained corrections were made in the reaction results for that day. No attempts were made to modify the reading time of the chronoscope by adjustment of the springs or of current.

The control interval employed as measured by the spark chronograph was 115.6 $\sigma$ . The reacting room was separated

from the experimenter's room by two hallways and three doors. The sound of the running of the chronoscope and the falling of the hammer was not perceptible to the reactor. The reacting room, however, was not quite soundproof, and there was occasionally some slight disturbance owing to persons passing in the hall outside, to the banging of doors, etc. If disturbed by such occurrences while reacting the subject signalled the experimenter by the means described below, and the reading for that reaction was struck out of the final records.

A warning signal was arranged in the following manner: A 16 c.p. incandescent lamp was placed in the reacting room. About two seconds before the coming of the stimulus this light was turned out by a switch in the experimenter's room, and this extinguishing of the lamp served as the warning to the subject. In series with this lamp in the reacting room, there was placed in the recording room a sound hammer, and a group of three lamps in parallel. By this arrangement the three lamps in the recording room were dimly illuminated when the lamp in the reacting room was brightly illuminated. When the subject wished to communicate with the experimenter he short-circuited his lamp by means of a push button, and in so doing caused the sound hammer in the recording room to fall and the three lamps to flash out at their full brightness.

The sound hammer was not released until the current was completely cut off (in giving the following warning to the reactor) and hence served only to call the experimenter's attention to the reactor's signals; these signals therefore were interpreted by the number of flashes of which they consisted. One flash meant that some disturbance had distracted the subject's attention at the moment of reaction, two that the apparatus was not working well, and three flashes that the subject wished to speak to the experimenter. In the first part of the experiment a telephone whereby the experimenter could converse with the subject was included in the outfit, but was later abandoned, as it was used very little, and being connected to the 'phone which conveyed the stimulus to the subject's ear caused some trouble by a "humming" which was occasioned by induction.

The procedure to record a reaction was as follows: Key I. on the Fall-hammer having been placed at the correct height, the main switch SI. was closed (to avoid heating the coils of the chronoscope it was kept open except when an experiment was actually being made) the chronoscope mechanism was started, the warning signal was then transmitted to the subject's room, and about one and a half seconds later the hammer was dropped. In falling it first opened contact ACI, starting the chronoscope, and at the same instant closed contact BCI, cutting in the interrupter. Then the hammer hit Key II and opened contact ABII and by so doing cut out the interrupter. Upon the reactor pressing the reacting key the chronoscope circuit was closed and the recording hands stopped. The chronoscope mechanism was then stopped, the reading was taken, and the whole procedure repeated.

The reacting key used in these experiments was the one described by Dr. Dunlap in the Psychological Review Monograph Supplements.<sup>22</sup>

The key was so adjusted that the slightest quick movement was sufficient to break the current through it, but so that the circuit would not be broken by the slower depression due to the weight of the hand. The actual weight required to depress the key was between 90 and 100 grams. A steady pressure of this weight did not break the contact.

#### 1. *Reactions to Five Durations of Stimuli.*

Five subjects were used in this portion of the experiment. D. is Dr. Dunlap, W. is the only woman who took part, and was a wholly inexperienced reactor. U. and J. are graduate students in the department of Psychology of the Johns Hopkins University, and have had some experience in reaction work, but not very much. Wh. was a freshman in the College and entirely inexperienced in psychological work. D., of course, understood the problem thoroughly, and U. and J. had some idea of it. The other reactors were wholly in the dark as to the meaning of their work. It was impossible to perform the experiments at a uniform time of day, owing to conflicting calls on the subjects

of class and other duties. The reactions were obtained at such times as the reactors were free. A series of fifty reactions was taken at each sitting, ten to each length of stimuli. A short rest was then given after which fifty more reactions were made, the stimulus-durations being given this time in reverse order. Before recording any reactions several series were taken to get the subject into the proper mode of attention, and to accustom him to the general routine of reaction, and to the handling of the key.

Two sets of instructions were used, one emphasizing the sensory, and one the motor form of attention. A synopsis of these instructions was typewritten and handed to the subject and they were orally elaborated at the beginning of the series of experiments, and on various occasions thereafter until they were pretty thoroughly understood.

It was desired that some of the subjects should exercise sensory and some motor attention. D. J. and Wh. were instructed (1) in the interval between reactions to consciously foster the intention to press key as soon as stimulus appeared, such deliberate intention only being necessary during early part of experiment, as, later on, the fingers would perform their duty without any such attention being exercised even before the warning signal. (2) That from the very first the fingers must be absolutely neglected as soon as the warning was received, the entire attention being put on the expected stimulus. On the other hand W. and U. were instructed (1) during intervals between reactions to remember that the coming of the stimulus should be the signal for pressing the key. (2) As soon as the warning signal comes to neglect the stimulus entirely and think only of pressing key as rapidly as possible.

These instructions were fulfilled with great accuracy as soon as the subjects were entirely familiar with the process of reaction. Frequent inquiries as to attention were made during both the training series and the experimental series proper, and were continued until the experiment was finally completed.

Each subject had but one set of instructions given him, and

was never required to change from sensory to motor type, or *vice versa*.

The question of the type of attention of a subject is an important one. Lange<sup>40</sup> first stated that the reaction time is shorter if the attention is directed to the expected movement rather than to the expected stimulus.

Wundt<sup>60</sup> reports that the difference in the time of reacting under the two conditions of attention varies, in the case of reactions to sound from 89σ to 114σ, and in the case of reactions to light from 109σ to 118σ. Martius<sup>43</sup> agrees in substance. Titchener ("Text Book" p. 432) says that the range of variation in reaction time due to the type of attention is about 1/10 of the total sensory time and 1/12 of the total muscular time.

Külpe (Outlines, p. 408) agrees that the sensory reaction lasts about 1/10 longer than the muscular reaction.

Flournoy,<sup>29 30</sup> Cattell<sup>17</sup> and Baldwin<sup>6</sup> refuse to agree that the sensory reaction *per se* is longer than the motor, Baldwin explaining the difference in time when it exists by the differentiation of mental types into visual, kinaesthetic-motor and visual-motor.

In the Johns Hopkins Laboratory the so-called sensory reactions with crude key and instructions, have been found in general to be longer than the motor.

We have called the kind of attention suggested by the first set of instructions given above "sensory". By some psychologists it would, perhaps, not be admitted that the attention they call for is "sensory" attention. But under these instructions the reaction time becomes very rapid,<sup>25</sup> the direction of attention is easy to maintain, and the whole reaction movement approaches an automatic form having few complexes and distractions. It would seem, therefore, that, given enough practice that reaction times under "sensory" and under "motor" attention approximate very closely.

A warning signal, described above, was sent about two seconds before the stimulus. The subjects were told that the interval between the warning and stimulus was not exactly two seconds

in every case, but would vary somewhat. This did away with rhythmic reactions.

The five lengths of stimuli were produced as described above by varying the rear contact of the Wundt fall-hammer. They were carefully timed by the spark-chronograph method. They were respectively (S5)  $7\sigma$ , (S4)  $30\sigma$ , (S3)  $51\sigma$ , (S2)  $76\sigma$  (S1)  $106\sigma$  in duration. The shortest was clearly perceptible as a single click, the others seemed to be not entirely homogeneous in their nature, that is, a slight variation in intensity was perceptible. The subjects were of the unanimous opinion that no difference in intensity was perceptible in passing from (S4) to (S3), to (S2), or to (S1), or in the reverse direction, but several of them claimed that there was a difference in intensity noticeable in passing from (S4) to (S5) or from (S5) to (S4). It may be objected that on this account *i.e.* because (S5) differed from the other stimuli in intensity as well as in duration, the reaction times to stimulus (S5) should be omitted from our final curves and tables. We have not so omitted them, although there may be some ground for the objection. However, the comparison of the results from (S5) with those from the other stimuli would seem to reveal no such disturbing factor. Stimulus (S4),  $30\sigma$ , was the shortest stimulus which could be obtained which showed no decrease in intensity to any of the subjects.\*

The experiment would have served most of its purposes if no shorter stimulus than this had been used. But for purposes of comparison it was deemed advisable to require all the subjects to react to a very much shorter stimulus.

D. gave, in all, 1000 reactions. These reactions are tabulated in Table I, being arranged as averages of sub-series of ten reactions each. That is to say that each value recorded in the table is not an actual reaction time, but is the average of ten

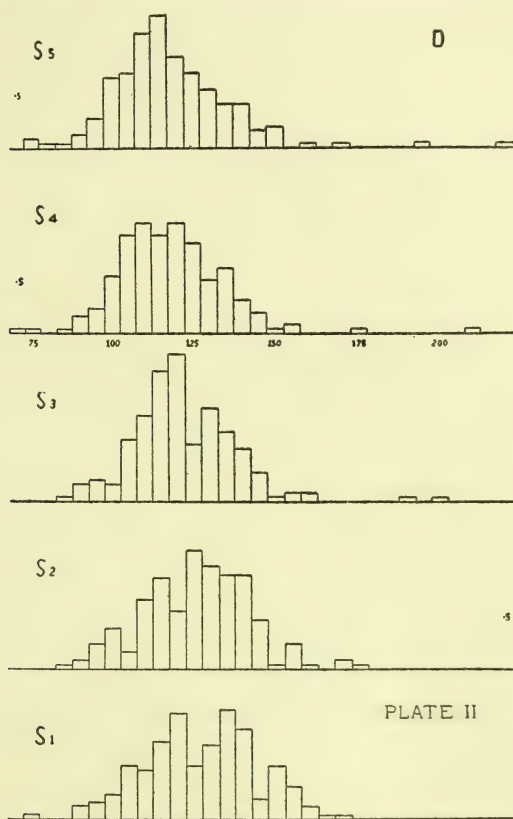
\* This stimulus is shorter than the duration which it is usually declared is necessary for the sensation of tone to reach its full intensity. Sander<sup>46</sup> found .6 sec. was the minimal time for strong auditory stimuli to produce a sensation of full intensity. Kafka<sup>37</sup> working with weaker intensity found the time required to be even longer, namely, 1.5 sec.

reactions. This is the case with all other tables in the experiments with auditory stimuli.

The mean variation in each case is not the average deviation of the averages of these groups of ten from the general average, but the average of the deviations of the individual reactions from the general average. For this reason the M. V.'s cannot be verified from the tables here published, but only from the original full records.

In all cases the figures in the tables represent measurements in *sigma*.

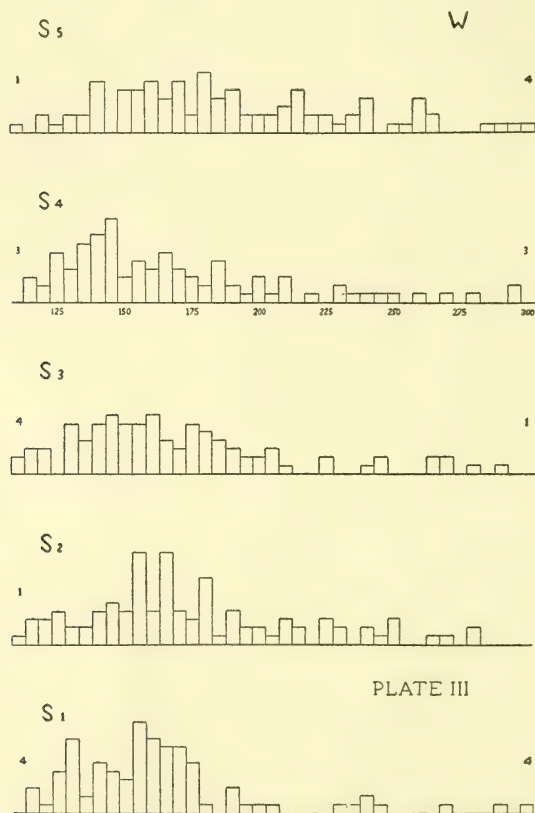
The reactions of D. are graphically represented in Plate II.



The ordinates represent the numbers of reactions, and the abscissæ the length of reactions.

As we have said, D. was the only experienced reactor taking part in the experiment. His reaction times are small and the mean variations are steady.

W. gave in all 500 reactions, 100 to each length of stimulus. They were divided and recorded in tens as in the case of D. The M. V.'s were computed in the same way, namely, on the variations of the individual reaction times and not on the sets of tens. This method was followed in computing the M. V.'s for all subjects. W. reacted 100 times to each duration of stimulus. W.'s reaction times were large, extremely so, and varied enormously. Her M. V.'s accordingly are very large. The graphs of her reaction times are given in Plate III and the figures in Table II.



U. gave 250 reactions in all, evenly divided among the five stimuli. See Plate IV and Table III. His reaction times are



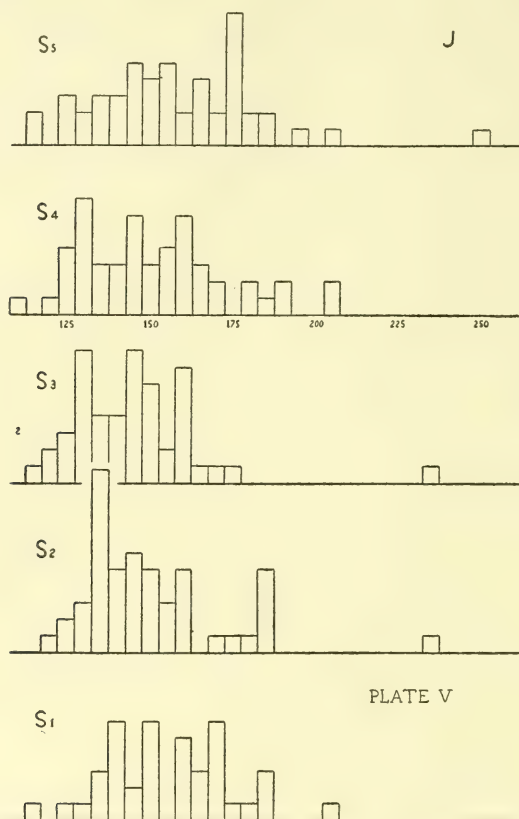
quite long, and his M. V.'s in most cases large, but only moderately so in the cases of stimuli (S<sub>4</sub>) and (S<sub>2</sub>).

Two hundred and fifty reactions were obtained from J. His times also are moderately large, his M. V.'s, with the exception of those to stimulus I, being small, although not so small as in the case of D. See Plate V and Table IV.

Table V and Plate VI show figures and graphs of 1000 reactions from Wh. His reaction times are moderately short, while his M. V.'s are somewhat large.

These data show that three out of five subjects, namely, W., U. and J. take longer to react to stimulus (S<sub>5</sub>) than to any of

the other four. In the case of W. there is a difference of  $6.1 \sigma$  between the most widely separated of her averages to the other four stimuli.



With U. the reactions to stimuli ( $S_4$ ), ( $S_3$ ), ( $S_2$ ), and ( $S_1$ ) are within  $10.3 \sigma$  of each other.

The averages of D. vary maximally by  $9.1 \sigma$ . J. shows a slightly wider divergence, there being a difference of  $14.8 \sigma$  between ( $S_5$ ) and ( $S_3$ ), ( $S_3$ ) being the shortest of all. The averages which Wh. gives differ by  $8.7 \sigma$ : In none of these cases is there a progressive increase or decrease of reaction time as the subject passes from stimulus to stimulus. In the opinion of the writer the differences noted are practically insignificant. There is greater disparity between reaction-times in every column of the

tables than between the averages of those columns. The likeness of the total average reaction times to each other is clear. These data demonstrate that when intensity difference is not present the duration of an auditory stimulus does not materially affect the time of reaction.

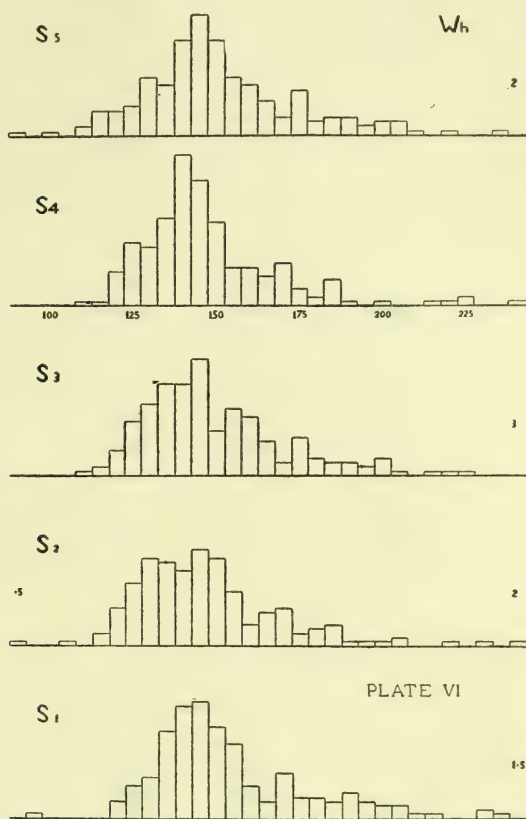


TABLE I. SUBJECT D

1000 Reactions

S1 106 $\sigma$	S2 76 $\sigma$	S3 51 $\sigma$	S4 30 $\sigma$	S5 7 $\sigma$
118.1	120.7	134.9	118.2	127.8
135.6	134.1	137.8	133.5	107.6
135.1	135.3	134.3	142.1	147.2
136.6	132.1	125.3	121.8	125.8
140.1	133.2	130.4	132.4	138.0
132.0	139.9	123.3	125.7	127.2
144.4	145.1	141.7	135.6	129.4
122.4	118.7	119.0	116.1	118.6
119.3	116.9	115.3	113.0	109.2
115.1	114.4	109.6	106.5	112.2
112.2	116.1	110.8	113.9	110.4
110.3	108.8	113.5	108.4	110.8
110.6	109.2	99.0	113.6	111.1
133.8	131.2	121.3	117.0	109.4
137.2	130.2	117.7	101.9	125.2
115.4	126.1	119.0	111.6	114.6
118.7	115.9	108.9	109.6	110.2
147.9	138.5	132.3	125.5	116.6
155.7	143.6	141.1	118.2	119.6
139.6	137.4	138.5	134.2	135.6
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Average 129.0	127.3	123.6	119.9	120.3
M. V. 13.7	12.07	12.1	12.4	12.7

TABLE II. SUBJECT W

500 Reactions

S1 106 $\sigma$	S2 76 $\sigma$	S3 51 $\sigma$	S4 30 $\sigma$	S5 7 $\sigma$
163.9	223.7	197.6	253.5	280.1
216.4	212.6	169.8	186.7	231.3
215.8	159.1	168.1	147.7	235.0
182.3	202.7	230.1	209.2	182.4
147.5	156.3	143.8	151.2	158.4
140.8	130.1	127.9	146.4	140.3
160.5	179.9	174.2	168.3	183.2
167.1	168.2	156.1	150.8	174.0
140.3	161.5	170.4	162.3	201.4
158.8	160.7	164.9	148.4	188.1
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Average 169.3	175.4	170.2	172.4	197.4
M. V. 33.8	31.0	33.6	36.5	42.9

TABLE III. SUBJECT U

250 Reactions

S1 106 $\sigma$	S2 76 $\sigma$	S3 51 $\sigma$	S4 30 $\sigma$	S5 7 $\sigma$
125.6	139.5	163.0	148.7	156.2
158.3	163.8	168.6	139.8	168.6
130.1	128.9	135.5	148.1	166.2
148.4	144.9	153.6	158.3	159.7
196.8	147.7	155.2	158.8	157.3
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Average 151.9	144.9	155.2	150.7	161.6
M. V. 26.8	14.7	20.0	15.9	21.3

TABLE IV. SUBJECT J

250 Reactions

S1 106 $\sigma$	S2 76 $\sigma$	S3 51 $\sigma$	S4 30 $\sigma$	S5 7 $\sigma$
172.5	171.7	158.4	157.3	150.4
169.8	164.6	164.6	172.6	180.1
143.7	144.7	134.6	161.6	168.3
150.6	142.3	136.4	135.6	163.6
154.0	141.3	132.7	138.5	138.0
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Average 158.1	152.9	145.3	153.1	160.1
M. V. 13.	16.2	14.8	17.2	19.3

TABLE V. SUBJECT Wh

1000 Reactions

S1 106 $\sigma$	S2 76 $\sigma$	S3 51 $\sigma$	S4 30 $\sigma$	S5 7 $\sigma$
167.9	163.7	169.2	146.1	149.4
194.5	187.2	195.4	162.9	199.1
163.6	143.9	181.0	154.9	154.0
155.3	147.5	145.2	146.6	171.5
156.8	149.1	189.2	156.5	161.6
183.7	145.2	150.8	148.6	151.1
188.0	166.3	136.4	167.2	168.3
145.9	129.7	157.8	154.2	149.7
144.2	140.3	147.7	139.6	157.6
143.6	151.2	149.3	131.8	149.7
146.2	151.4	149.4	141.7	159.1
166.7	143.7	132.5	133.5	135.2
142.8	134.1	135.6	156.8	149.0
144.9	157.0	151.4	159.5	159.5
163.5	153.5	152.9	154.0	150.4
153.0	147.4	151.3	148.1	147.1
144.2	142.3	141.6	131.8	143.1
146.2	144.9	150.0	143.6	144.4
138.2	137.6	136.2	139.0	156.2
162.5	149.5	164.0	160.3	170.2
<hr/>				
Average 157.5	149.2	154.3	148.8	156.3
M. V. 19.03	17.7	20.29	14.05	20.00

2. *Reactions to Auditory Stimuli of Varying Durations, with Previous Training in Reacting of those Stimuli*

In part 1 of this experiment the subjects before making any reactions to be recorded, underwent a period of training in reacting to all five durations, and, as just stated, their reaction-times for the four longer stimuli show no uniform differences under these conditions. The next step was to find out whether or not if they were trained to react to a stimulus of a certain length they would subsequently react in the same manner to stimuli of other lengths. Two series of experiments were performed to settle this point. In the first series two men were trained to react to one stimulus, and then were made to react to all five stimuli. In the second series two subjects were trained to react to a certain stimulus and later reacted to one other stimulus of different length.

A. *Previous training to one length of stimulus and reacting to all five durations of stimuli.*

The subjects, two in number, were given a somewhat lengthy training to a particular stimulus duration. When their reacting had become more or less easy and steady they were presented with stimuli of five lengths, exactly as in Experiment 1. The stimulus durations employed were the same as in Experiment 1.

Two subjects were used here, We. and Do., both under-graduates in the Johns Hopkins University, neither were previously practiced reactors. We. was trained with 600 reactions to stimulus S1 (106σ).\*

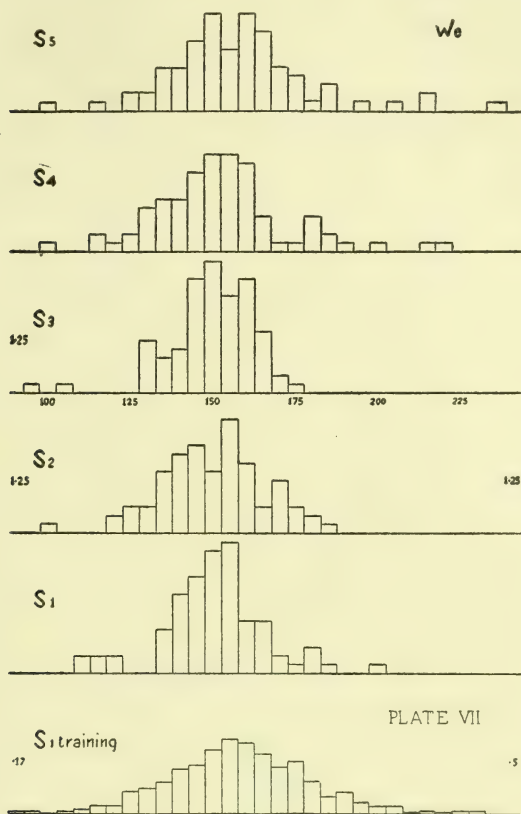
His averages and the M. V. for the 600 reactions, arranged in groups of ten each and averaged by hundreds are given in Table VI. The graph plotted from these results is given in Plate VII.

We was next given a series of the five lengths of stimuli arranged exactly as in Experiment 1. He reacted 80 times to each

\*In the case of both We. and Do. and in the cases in Experiment 2B following the number of "training" reactions does not include some preliminary trials in which the subject made himself familiar with the working of the key, and the general process of reacting. These preliminary trials consumed a day or two. No record was taken of them.

length of stimuli or 400 times in all. These average and mean variations are given in Table VII and the graphs plotted from the series in Plate VII.

Do., was trained with stimulus S<sub>4</sub> (30σ). He reacted 400 times and his averages with M. V. are given in Table VIII. See Plate VIII for the graph plotted from these figures. He was



then presented with the five lengths of stimuli in sets of ten each, 110 reactions to each duration, 550 in all, precisely as was done to the subjects in Experiment 1, and to We. in this Experiment. See Table IX and Plate VIII for the averages and graph.

TABLE VI. SUBJECT We  
Stimulus S1 106 $\sigma$  600 Reactions

		161.4	
170.5		158.4	
193.6		156.5	
191.3		153.9	
188.		150.7	
164.1		151.8	
162.2		157.5	
163.8		152.6	
162.3		152.7	
176.3		152.1	154
178.8	175	159.7	
177.5		151.2	
187.		157.9	
173.6		151.	
175.6		143.6	
182.7		148.4	
172.4		143.8	
168.5		149.4	
165.5		160.6	
174.5		145.7	151
174.4	175	138.6	
163.4		133.2	
165.		144.7	
158.9		145.2	
166.3		149.2	
166.2		158.5	
187.4		142.5	
158.2		137.5	
186.		140.	
157.		142.6	143
173.9	168		
		Average 161.	
		M. V. 16.7	

TABLE VII. SUBJECT We  
400 Reactions

S1 106 $\sigma$	S2 76 $\sigma$	S3 51 $\sigma$	S4 30 $\sigma$	S5 7 $\sigma$
142.5	134.8	144.3	145.5	147.1
147.9	151.9	149.7	155.	156.3
146.9	148.4	148.1	140.	156.8
154.4	151.4	146.	160.	175.2
146.	149.9	147.6	143.3	164.
162.	159.6	160.	162.1	179.8
153.2	172.6	151.2	151.9	148.7
162.2	158.4	159.7	163.6	152.3
Average		150.8	152.7	160.05
M. V.	11.6	10.3	14.9	15.0

TABLE VIII. SUBJECT Do  
Stimulus S4 30σ 400 Reactions

154.2		182.4	
148.6		171.7	
148.3		176.7	
156.8		188.5	
173.4		182.6	
159.8		177.3	
162.4		183.2	
189.6		165.9	
203.2		160.9	
179.5	167	178.9	176
184.4		144.7	
184.7		133.3	
181.6		142.9	
174.5		145.	
171.2		157.4	
171.5		152.0	
190.1		164.4	
182.5		160.5	
198.5		153.4	
184.9	182	148.8	150
<hr/>			
		Average	168.75
		M. V.	19.5

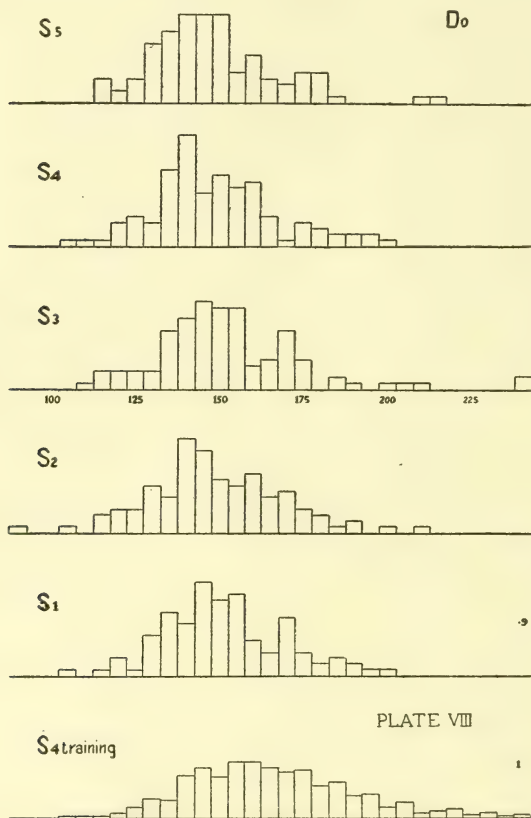
TABLE IX. SUBJECT Do  
550 Reactions

S1 106σ	S2 76σ	S3 51σ	S4 30σ	S5 7σ
174.7	147.5	151.5	143.5	138.
146.2	136.3	140.1	140.8	160.6
152.3	152.2	149.4	158.	147.7
148.2	159.1	144.3	140.1	140.6
147.9	144.1	156.7	154.2	155.7
156.4	159.5	149.9	152.4	154.2
152.9	161.9	149.	141.9	143.8
143.4	145.4	167.2	145.4	139.1
155.3	150.2	158.6	155.3	150.3
173.1	166.5	156.2	168.8	160.4
154.1	162.	179.2	169.5	164.6
<hr/>				
Average	154.8	153.1	154.7	151.8
M. V.	14.8	17.5	15.7	14.5
				13.8

It may be seen from these results that there is no significant effect of varying the stimulation-duration, even when the subject has been lengthily trained in reacting to one duration.

B. *Previous training in reacting to one length of stimulus and subsequent reacting to one stimulus differing greatly in length from the one used in the training.*

Two subjects were each trained in reacting to one length of stimulus exactly as in A of this experiment. The same two stimuli durations were chosen. After the training period a series of



reactions was taken to a single stimulus differing in length from the one used in the training process.

The first subject, G., is a graduate student in the department of Philosophy, having had a small amount of experience in psychological experimentation. He was trained to react to the longest of the five stimuli which had been used in experiment I, namely

stimulus  $S_1$  ( $106\sigma$ ). His average for 450 reactions was  $151.7\sigma$  with M. V. of  $14.7\sigma$ . He then reacted 450 times to stimulus  $S_4$  ( $30\sigma$ ). The average here was  $154.4\sigma$ . These averages are gathered together in Table X. The graphs for these two series are inserted for purposes of comparison in one plate, namely Plate IX.

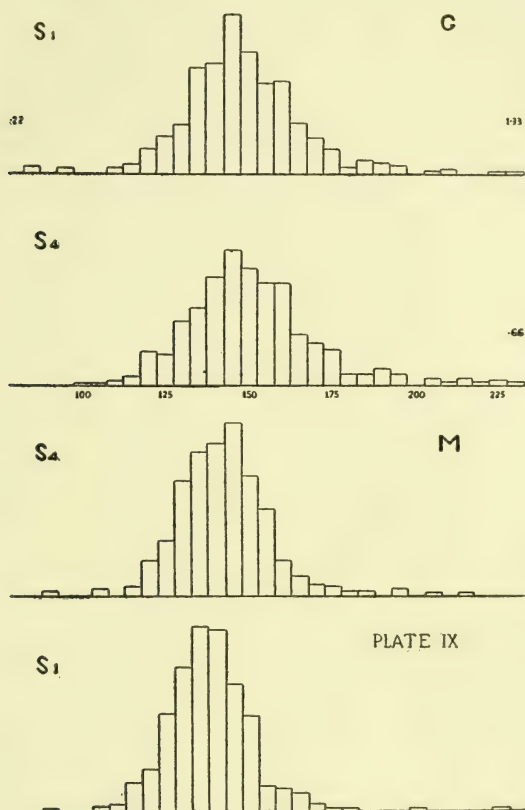


TABLE X. SUBJECT G

450 Reactions Stimulus S1 106 $\sigma$	450 Reactions Stimulus S4 30 $\sigma$
158.7	168.3
154.8	143.4
153.9	158.4
147.4	161.0
139.6	151.2
149.2	163.0
145.7	145.6
141.1	149.3
142.1	146.0
142.6	147.0
156.3	157.5
151.7	144.6
133.5	153.6
148.2	162.4
146.3	172.5
141.1	162.8
139.9	157.1
132.9	167.3
141.5	159.8
152.8	162.6
168.3	150.7
170.5	153.9
153.6	155.7
157.9	151.6
150.6	158.1
171.4	169.3
151.1	155.0
151.8	147.7
167.0	151.5
158.9	157.9
166.0	164.2
149.3	149.6
140.1	147.0
154.5	141.7
142.6	149.0
156.4	152.5
157.2	141.0
156.5	163.6
152.3	164.1
155.4	148.2
143.6	149.5
161.8	151.0
166.3	149.0
153.9	133.3
150.1	152.2
<hr/>	
Average 151.7	Average 154.4
M. V. 14.7	M. V. 15.2

TABLE XI. SUBJECT M

450 Reactions Stimulus S <sub>1</sub> 106 $\sigma$	450 Reactions Stimulus S <sub>4</sub> 30 $\sigma$
145.6	143.5
158.2	139.9
151.7	135.8
142.5	144.6
151.2	149.1
137.7	152.5
131.8	158.4
131.6	145.6
139.9	149.7
138.3	153.7
139.4	154.2
136.2	138.8
130.2	144.0
136.8	146.3
135.7	142.1
138.1	150.2
144.8	140.1
153.4	140.2
147.2	132.5
142.0	134.6
146.6	144.2
146.6	142.3
145.6	129.5
142.8	141.1
141.0	128.9
146.7	152.0
144.8	153.9
136.9	142.7
132.6	143.8
130.0	138.7
139.3	153.8
142.4	147.9
130.4	147.6
157.5	130.4
134.7	132.5
131.3	151.7
135.7	151.5
136.4	142.8
138.2	155.9
134.0	145.1
136.2	149.2
133.0	157.2
148.1	145.9
145.1	143.9
134.2	139.0
<hr/>	
Average 140.4	Average 144.6
M. V. 9.08	M. V. 10.4



### III. REACTIONS TO VISUAL STIMULI OF VARYING DURATION

After the completion of the work with auditory stimuli two series of experiments were made with different lengths of visual stimuli. In one series the stimuli used were five durations of illumination preceded and followed by darkness; the stimuli of second series consisted of five durations of darkness, of approximately the same lengths as the times of appearance, preceded and followed by illumination. These stimuli will be referred to below, in the cases of the occlusion or limited darkening of the light as A<sub>1</sub>, A<sub>2</sub>, A<sub>3</sub>, A<sub>4</sub>, A<sub>5</sub> and in the cases of the presentation of the light as B<sub>1</sub>, B<sub>2</sub>, B<sub>3</sub>, B<sub>4</sub>, B<sub>5</sub>. A<sub>1</sub> and B<sub>1</sub> were what may be called total occlusion and total presentation, that is to say, in these cases the light was respectively presented or occluded for a length of time very much longer than any simple reaction time. The length of A<sub>1</sub> and B<sub>1</sub> was one second, just half the period of the pendulum. The other stimulus lengths, carefully measured by a drum chronograph with 250 d.v. fork and a Pfeil marker were in sigma, A<sub>2</sub>, 150; A<sub>3</sub>, 66; A<sub>4</sub>, 31; A<sub>5</sub>, 10; B<sub>2</sub>, 144; B<sub>3</sub>, 64; B<sub>4</sub>, 25 and B<sub>5</sub>, 12.

The source of illumination was a Nernst filament mounted in the lamp-box of a projection lantern. The filament was started by means of a small gas flame. The lantern lenses were so arranged that an image of this filament was focused at a point 167 centimetres from the filament, and 70 centimetres from the front lens of the lantern. The pendulum, which has been described by Dr. Dunlap,<sup>26</sup> was placed so that the image was focused on the screen carried above the knife edges. Beyond the screen, when it was removed, the diverging rays were projected upon a plaster of paris disc 9 cm. in diameter, placed 162.4 cm. from the screen and 232.4 cm. from the front lens of the lantern. The background surrounding the disc was of black velvet, which absorbed all rays falling upon it. The room was dark

and a large screen was placed between the subject and the pendulum, cutting off the faint illumination from the image on the pendulum screen so that the illuminated disc shone out very brightly. It can be seen that a screen cut with edges on radii from the knife edges would occlude the light from the disc for a period of time which is a function of the width of the sector, the time of swing of the pendulum being constant. Five such screens were used for the A series (the occlusion of the light). For the B series, in which the light was exposed, five screens were used with apertures cut on radial lines. These screens in all cases but one were made of cardboard. In that one case, A<sub>5</sub>, a very narrow sector was required, and to prevent any chance of its being broken it was made of aluminum.

The subject was seated beside the pendulum about two metres from it. As has been said, a screen was placed between him and the pendulum and lantern. The distance from the eye of the subject to the disc was about three metres. No head rest was used and this measurement is therefore only an approximation, the variations from it being, however, small and negligible. The plane of the disc was not exactly perpendicular to the axis of the light, but was turned slightly toward the reactor. The disc was really a circle of 9 cm. diameter but as seen by the subject was an oval 9 cm. high and about 8 cm. wide.

The retinal image of the plaster disc for the normal eye at the distance given is about .4 mm., according to the simple formula  $A : B :: a : b$ , A being size of object, B size of retinal image, a, being distance of object from nodal point of eye, and b, distance of retina from nodal point. According to Howell ("Physiology" p. 306) b equals 15.5 mm. Since the diameter of the fovea is approximately .341 mm. (Howell "Physiology" p. 307) with exact fixation the whole fovea would be covered. Slight divergence in fixation would stimulate a considerable foveal area, and some portion of the extra foveal macula. This probably provides the most nearly uniform stimulation which can be secured.

The subject used both eyes, and as the disc was about 45 cm. above the level of the eye, his gaze was slightly upward.

In using the Nernst glower as a source of illumination two points must be kept in mind if its luminosity is to be maintained constant. In the first place the illuminating efficiency of the Nernst varies considerably according to the length of time it has been used. Yerkes and Watson<sup>62</sup> report that the life of a Nernst glower is six hundred hours on direct current and eight hundred hours for alternating current, regardless of position.

During the first two or three hundred hours the intensity of the light diminishes rapidly as much as ten or fifteen per cent. Then follows a period during which the intensity of the light remains practically constant.

In our experiment a new glower was allowed to burn for several hours before being used to produce the stimulus light.

The illumination at the point at which the disc was in the beam of light was carefully determined by the Lümmer-Brodhun photometer, and was equal to that of a  $3\frac{1}{4}$  c.p. lamp at a distance of 142.5 cm. This measurement is within 2% of exact accuracy. The intensity was kept constant by the following procedure.

Between the sector or focus of the rays and the disc a Lümmer-Brodhun photometer and a small standardized incandescent lamp were placed. These two pieces of apparatus were carefully adjusted so that the illuminations of the standardized lamp and of the stimulus light exactly balanced. They were fixed in position so that they did not vary laterally, but could be lowered out of the path of the light during reaction experiments, and returned to the position for controlling the intensity at any time. The photometer so used did not give a measure of the intensity, but simply a means of keeping it constant.

The second point to be kept in mind is that to maintain a constant luminosity on the Nernst filament it is necessary to maintain a constant current supply. The characteristics of the Nernst glower as a pyroelectrolytic conductor are such that constant voltage at the glower terminals does not insure even approximately constant illumination. (Steinmetz.<sup>53</sup>) Therefore it is necessary to control the amperage accurately. The

current through the Nernst glower which we used was controlled by a rheostat, and an ammeter was kept so that it could be switched into series with the glower. The amperage necessary to keep the light up to its normal intensity varied from .6 amperes to about .65 amperes. The test with the photometer was made at frequent intervals, the ammeter reading was taken several times during each working period. In this way a complete check was obtained on the intensity of the stimulus light.

The lantern, exposing apparatus and the subject were in one room, the recording apparatus and experimenter in another.

Plate X represents schematically the apparatus for producing the light, the reaction key and the apparatus for registering the reaction time. The photometer and control lamp are not shown.

A represents the lantern, B and C respectively are the rheostat and ballast for controlling the Nernst filament. D is the ammeter and E1 and E2 are switches by means of which the ammeter may be cut in or cut out of the circuit. F1 and F2 represent the two contacts on the scale of the pendulum. These contacts are actuated by a permanent magnet on the swinging arm of the pendulum. In F1 is also a permanent magnet but of less strength than that on the swinging arm. Thus a and b were always kept in contact except when the pendulum swung over them. Immediately after the pendulum has passed over a and in so doing has broken the contact ab the contact is re-established. The time necessary for the re-establishing of this contact was about 25 $\sigma$ . F2 was used only for checking purposes and at other times was slid entirely beyond the reach of the pendulum magnet. In F2 c, d were always kept in contact while recording reactions. H1 and H2 are the upper and lower magnets of the Hipp chronoscope which was used for the recording. The chronoscope was used without springs in the manner adopted in the Johns Hopkins laboratory.<sup>24</sup>

The circuit as represented in Plate X goes from the terminal T, to F2, where it divides. One branch goes to F1, thence to the upper magnets of the chronoscope H1, to the control key L, to the double throw switch 1 and back to the terminal T4. The

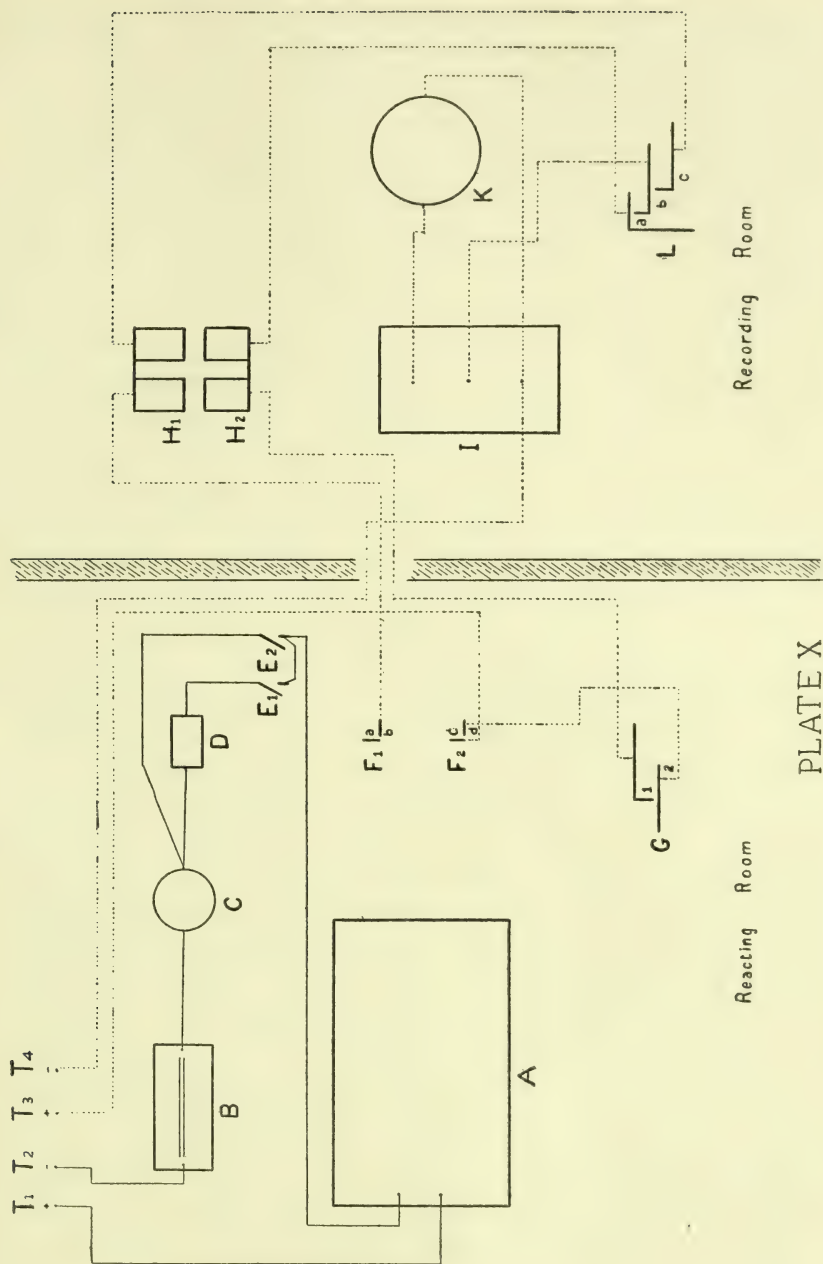


PLATE X

other branch goes from F2 to the reaction key G, to the lower chronoscope magnets H2, thence to control key L, to switch 1, and back to the terminal T4. The current which divides at F2 unites again at the key L.

The pendulum was held deflected by an electro-magnet and was released by a switch in the experimenting room. The diagram does not represent this switch. The circuit used was the direct current 118 volt power circuit, and was kept constant at .17 amperes.

The reaction switch G is the same one used in the auditory experimentation. It was kept closed at G1 G2, and was broken by the slightest decisive movement of the reactor's finger in the manner described above.

The control key L is one which has been used by Dr. Dunlap and myself in earlier reaction work<sup>25</sup> but was not described in our report and deserves a few words. The lever b moves on a fixed pivot and the lever c moves on a pivot attached to b, enabling the operator by a slight pressure of his finger to make c and b come into contact and by a greater pressure to bring b and a also into contact. Both levers are held down by the same spring (not shown), and by means of a second spring, not used in this experiment, a more complicated action of the levers may be obtained. It has been said that the current which split at F2 unites at L. This main current returns to the terminals from b. Thus it can be seen that when the experimenter presses his finger on this key he makes contact first between bc which actuates the upper magnet. This moves the armature of the chronoscope up, putting the hands out of action. When ba come into contact the armature remains against the upper magnet until the current through it be broken, when the armature moves over to the lower magnet. The current through the upper magnet is broken when the pendulum is dropped, the permanent magnet on the pendulum breaking the circuit through ab. This current through ab is re-established in the manner described above, but not until after the armature of the chronoscope has moved to the lower magnets. The current through these lower magnets is broken

by the reacting movement of the subject so that the armature immediately moves back to the upper magnets and the recording hands stop. This method of operating the Hipp chronoscope has been found extremely satisfactory.

For checking purposes F2 was placed at a determined place on the dial of the pendulum, the reaction key being kept closed. This arrangement provided that the swinging pendulum first broke ab and then cd. The breaking of the circuit at cd acted exactly as the breaking of the circuit by the reaction key at G. The duration of the swing of the pendulum between F1 and F2 was determined by the spark chronoscope, the period used for checking being  $158.75\sigma$ . The error of setting of F2 was below  $1\sigma$ , and the chronoscope averages from day to day never varied more than  $5\sigma$ , and seldom as much as  $3\sigma$ . This variation was doubtless due to effects of temperature on pendulum and chronoscope. The mean variations of the chronoscope readings were on every day less than  $1\sigma$ .

The warning system consisted of a buzzer in the subject's room and a sound hammer in the experimenter's room. This, for simplicity's sake, is not represented in the diagram. A key in the experimenter's room and a switch in the subject's room were connected with the buzzer and the sounding hammer by a three-wire system which enabled either experimenter or subject to signal to the other.

The process of reaction was as follows: The glower having been put in operation and the subject's eyes having adapted to the darkness, the experimenter closed Key 1, started the chronoscope mechanism and sent a warning signal to the subject. About a second and a half later the switch which dropped the pendulum was broken. The pendulum took half a second to swing to the lowest point of its arc, at which place the exposure (or occlusion) of the light was effected. Thus the interval between the warning and the stimulus was about two seconds. The moment of exposure was, of course, the moment at which the chronoscope hands began to run. Immediately after the subject reacted the experimenter lifted his finger from the key L, thus

breaking the current through the whole system and preventing the back swing of the pendulum from again starting the chronoscope hands. The pendulum was caught by the electro-magnet on its return swing.

The chronoscope was then stopped, the reading recorded and the operation as a whole repeated.

Eight subjects were used in this investigation. With two exceptions they were previously inexperienced reactors. In all cases they were given a lengthy series of reactions before any were recorded. This training series lasted in some cases about two weeks and was continued until any practice effect in future reactions was eliminated. It was planned to have each of the eight men react five hundred times to each of the ten stimuli. This program was carried out in six cases. In one case it had to be curtailed because of sickness, and in another for less excusable reasons for absence on the part of the subject. In both of these cases, when it was seen that the entire program could not be completed, it was thought advisable to have the subjects react to each stimulus a fewer number of times rather than to omit entirely the reactions to any one particular stimulus. Accordingly they reacted only two hundred times to several durations of stimuli.

It was not possible to have any particular time of day for experimenting. The work was carried on whenever the subjects could arrange to act. In some cases it was as early as eight in the morning and in others as late as four in the afternoon. In fact the work of experimentation was in progress for a period of some months during practically all of every day.

The reactions were taken in sets of fifty. The subjects were given a short rest after every fifty reactions. As a rule one hundred and fifty reactions were taken at a time, in a few cases two hundred were taken; never more.

The instructions to the subjects were very carefully considered. In order not to confuse two problems it was decided to have all instructed alike, and it was also decided to instruct all to react with "sensory" attention. Inasmuch as the problem

was to determine the influence of sensible differences on reaction time it seemed that those differences would be accentuated by being observed with sensory attention. It is conceivable that purely "motor" attention, assuming such to be possible, might tend to obliterate the effect of such sensory differences as we used. The instructions were typewritten and were handed to the subjects before every set of reactions until they were thoroughly understood. These instructions for the "A" series were as follows: (1) "While waiting for the 'buzzer' to sound reflect on the fact that if you watch attentively for the disappearance of the light your finger will press the reaction key immediately upon its disappearance, no attention or effort being directed to the fingers. (2) As soon as the 'buzzer' sounds place fingers on key (if they are not already there) and immediately withdraw your attention from them; attending strictly to your task of noticing the instant of the disappearance of the light. (3) After the reaction has occurred, relax. No attention need be paid to the light until the 'buzzer' sounds again.

"After you become used to the experiment you will find that practically no attention need to be paid to the fingers at any time; they will automatically find their place when the 'buzzer' sounds and will react at the proper time.

"The points which it is important to remember at all times are:

"A. That your determination to react immediately on perceiving the disappearance of the light must be made once and for all before beginning the series. If it is necessary to re-inforce this determination do so in the interval between the reaction and the next warning, as you are instructed in (1). *Do not think of the hand, or of the reaction while watching for the disappearance of the light.*

"B. After the warning watch attentively for the disappearance of the light. Try to see it at the very instant in which it occurs.

"The warning will come about two seconds before the stimulus, but not exactly two seconds before. The interval may vary slightly.

"After each series inform the operator concerning your feelings during the process of reacting, and *especially be sure to mention any unusual occurrence or experiences of any kind.*

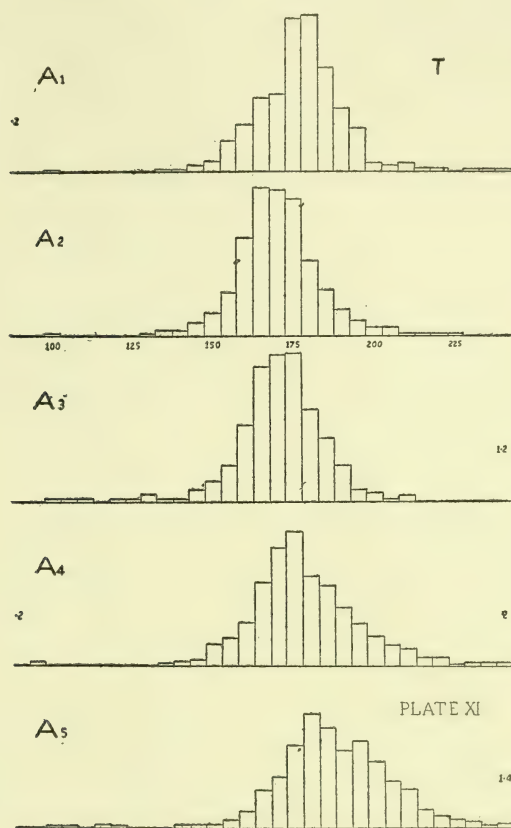
"Substitute 'appearance' for 'disappearance' when reacting to series B."

Very careful inquiries were made of the subjects from day to day to ascertain how fully the instructions were followed. As a matter of fact the instruction sheet proved entirely intelligible and its directions were fulfilled in every case almost to the letter.

As regards the cutting out of records of reactions which seemed to be unsatisfactory three simple rules were followed. The reactions which were excluded were: (1) Those which seemed to the *subject* to be abnormal, whether lengthened by inattention or outside disturbance, or anticipated. The information that they were abnormal was conveyed to the experimenter by means of the switch in the subject's room which was included in the circuit of the warning signal, and which switch operated the sounding hammer in the recording room. (2) Those which seemed to the *subject* to be abnormal *after an inquiry from the experimenter*, mediated by means of the buzzer, as to whether that particular reaction was correct or not. (3) Those which were over  $300\sigma$  in length, or shorter than  $60\sigma$ . These last were very few, and, as will be seen by reference to the graphs were unimportant..

T. is an undergraduate of the Johns Hopkins University in the freshman year. He reacted in full to all of the stimuli. He began reacting with A1 and proceeded in order to A5, then from B1 to B5. The averages of his reactions with their mean variations are given in Table XIII and the curves plotted from these averages in Plates XI and XII. In the table each of the figures is the average of fifty reactions and the mean variation is the average of the variations of the individual reactions from the average of the 500 reactions to that stimulus. In the reactions to the A series a phenomenon appears which is met with several times throughout the experiment. The reactions to the medium durations A2 ( $150\sigma$ ) and A3 ( $66\sigma$ ) are shorter than the reactions to

any other of the five durations.  $A_1$  ( $1^{\text{sec}}$ ) and  $A_4$  ( $31\sigma$ ) are very little longer, while that to  $A_5$  ( $10\sigma$ ) is much longer than any of the others. In the opinion of the writer a difference of six or eight sigma has practically no significance in measuring reaction times. It can be seen by reference to the table that



there are three averages in the  $A_1$  series, namely 186, 185 and 190 which have a good deal to do with bringing the total average to six sigma greater than the averages of  $A_2$ . In the case of  $A_4$  two or three high averages, especially one of 201, augment the total averages considerably. It is not a distortion of the truth to say that the reactions to the four longer occlusions are practically of the same length. This cannot be said of the reactions to

A5. Here there seems to be a real increase of reaction time. This stimulus was very short,  $10\sigma$ , and was seen as just a flick of shadow passing across the illuminated face of the disc. T. reported on one occasion that A5 was harder to react to than any of the others of the A series, and later said that he thought that the reactions to A5, "may be slower than reactions to longer stimuli because it seems to take time to be sure whether one has seen the light go out or not". This at once suggests that there may be some intensity difference between A5 and the others of the A series. It is not impossible that there may be such difference, although none of the subjects reported it specifically. It is more probable that some matter of eye movement or eye strain comes in here; that the very short occlusion held the eyes irresistibly for a space of time during which no reaction movement could be made. This matter of eye movement and also of eye strain was observed to be an increasingly powerful factor as the duration of stimulus decreased, but was very much more noticeable in the case of A5.

Reference to the graphs shows very even distribution for all but A5. Here there is a small secondary crest.

The averages for the B series, in which the light appeared show that B1 (1 sec.) gives the longest reaction time. B2 ( $144\sigma$ ), B3 ( $64\sigma$ ) and B4 ( $25\sigma$ ) give almost exactly the same time. B5 ( $12\sigma$ ) is the shortest of all by a good deal. This was probably the hardest of all the ten stimuli to react to. This fact undoubtedly acted in this case to increase the attention of this particular subject.

T. reported that taken as a whole, the B series was easier to react to than the A series. And B2 was the easiest of all. The graphs of the B series and particularly of B2 are less evenly distributed than those of A. This indicates that the attention unconsciously relaxed in reacting to the easier stimuli, causing a greater number of lengthened reactions. But when B5 was reached it is probable that its unexpected difficulty brought about an increase of attention and a resulting lower reaction time and more even distribution.

S. is an undergraduate of the Johns Hopkins University. He

completed the entire program, going, as did T., from A1 to A5 and from B1 to B5. The averages of his reactions as recorded in Table XIV show a very high degree of regularity. His reactions are uniformly slow. His best reactions was to A5 ( $10\sigma$ ). The reaction times to stimuli A1, A2, A3, and A4 are practically of the same length. That to A5 is about five *sigma* shorter. In the reactions to the B series that to B1 is the longest, that to B4 the shortest, and those to B2, B3 and B5 within three *sigma* of each other. There is not ten *sigma* difference between any two. When it is remembered that very slight difference of general health or of fatigue on the part of the subject, or difference of temperature in the reacting room may change the reaction time of a subject more than ten *sigma*, these results may be considered as alike. Furthermore, the smallness of these differences constitute a significant commentary on the absolute necessity of having a large number of reactions before any tabulation of them can mean anything at all. By choosing averages from any single day's work on the different stimuli almost any kind of result imaginable can be obtained. Generalizations founded on reactions which have not been spread over a considerable period of time may be entirely misleading.

The graphs drawn from these averages are shown in Plates XIII and XIV. The distribution is not even, but is no more uneven in any one case than another.

TABLE XIII.

Subject T. 5000 Reactions					
A1 (1 sec.)		A2 (150σ)	A3 (66σ)	A4 (31σ)	A5 (10σ)
	171	179	170	169	199
	177	170	169	175	193
	174	181	170	167	191
	186	173	174	189	185
	185	171	172	179	199
	171	177	175	189	196
	175	175	171	177	181
	178	167	178	181	189
	178	164	171	188	189
	190	169	172	201	177
Average	178.5	172.6	172.2	181.5	189.9
M. V.	7.54	13.85	12.69	21.27	14.02

B1(1 sec.)	B2(144 $\sigma$ )	B3(64 $\sigma$ )	B4(25 $\sigma$ )	B5(12 $\sigma$ )
178	183	170	176	151
166	177	172	173	150
168	189	172	168	158
183	173	181	167	155
181	165	150	169	154
165	164	170	180	157
176	164	169	153	150
184	159	164	159	144
168	170	172	170	146
197	174	171	163	145
<hr/>				
Average 176.6	171.8	169.1	167.8	151.0
M. V. 17.64	14.52	14.16	14.76	17.99

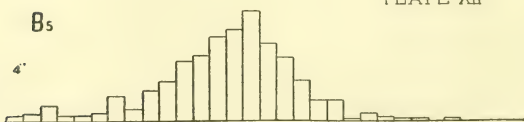
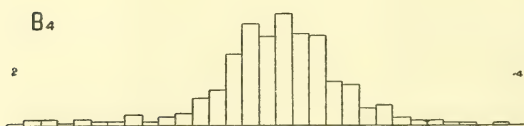
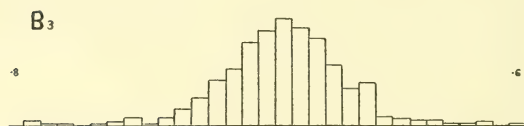
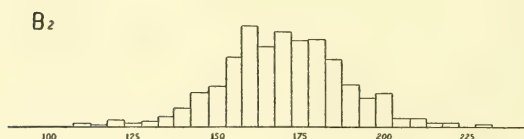
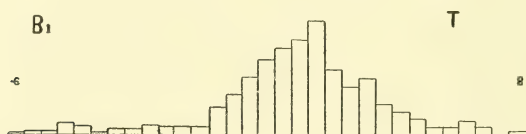


PLATE XII

TABLE XIV

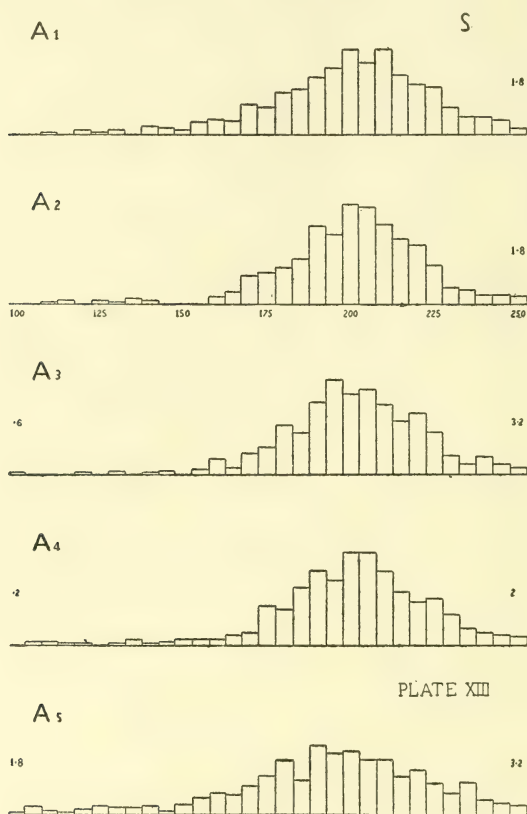
Subject S. 5000 Reactions

A1(1 sec.)	A2(150 $\sigma$ )	A3(66 $\sigma$ )	A4(31 $\sigma$ )	A5(10 $\sigma$ )
184	192	192	205	198
178	204	197	195	192
207	210	194	205	209
202	200	219	199	195
219	203	217	199	206
205	199	212	211	216
196	205	209	206	183
204	199	211	206	194
208	201	204	199	188
212	206	200	208	188
<hr/>				
Average 201.5	201.9	205.5	203.3	196.9
M. V. 17.53	15.83	23.63	17.87	25.52
<hr/>				
B1(1 sec.)	B2(144 $\sigma$ )	B3(64 $\sigma$ )	B4(25 $\sigma$ )	B5(12 $\sigma$ )
206	182	199	184	180
198	202	181	188	192
188	196	167	196	192
199	192	184	168	183
198	204	185	170	188
201	193	184	181	200
191	189	188	190	195
199	191	203	182	206
201	189	202	174	195
199	201	211	180	198
<hr/>				
Average 198.0	193.9	190.4	181.3	192.9
M. V. 22.75	18.96	22.22	19.99	18.65

De., a freshman of the undergraduate department of the Johns Hopkins University, also completed the entire program of five thousand reactions, responding in the order from A1 to B5. The averages of his reaction times are given in Table XV and are graphically represented in Plates XV and XVI.

It is difficult to make any generalizations on these averages. De. reported at the conclusion of the whole series that in general the shorter stimuli seemed easier to react to. The shortest average reaction of the two series is to A4 and B4 comes next. But note the difference in distribution of the reactions to these stimuli. The longest average is to A1 and the next longest to B2. De. said that in the cases of the longer stimuli there was a

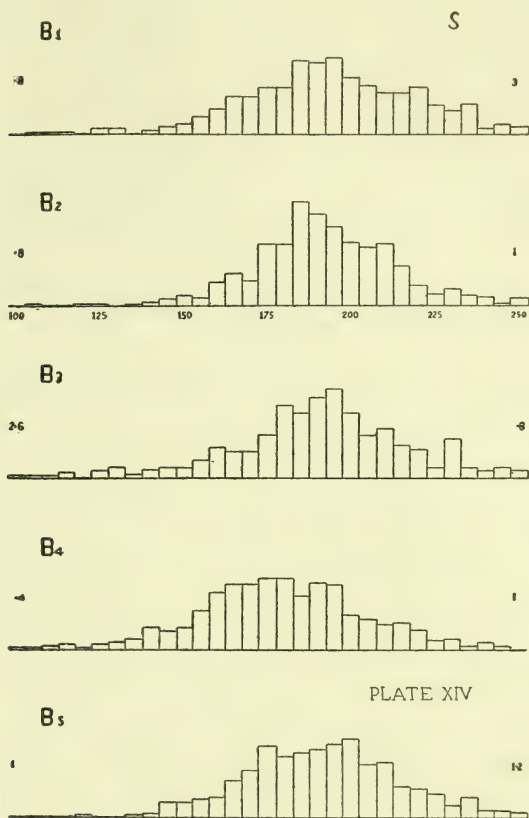
"kind of feeling that he had to wait". There is a difference of nearly twenty sigma between  $A_1$  and  $A_4$ , and of fourteen sigma between  $B_2$  and  $B_4$ . Many of the graphs are somewhat irregular.



D. is Dr. Dunlap, who also acted as a subject in the auditory part of this investigation. He reacted the full five thousand times, in the same order as T., S. and De. His averages are contained in Table XVI and the graphs from them in Plates XVII and XVIII. With D. in both series the extremely long and extremely short stimuli produced the slowest reactions.

$A_5$  is longer than  $A_1$  while  $B_1$  and  $B_5$  are about the same length. D. reported after reacting to  $A_5$  that the "reactions feel not at all different from the others. All these reactions seem

about the same. The first few after changing duration are discouraging. No change of intensity is felt. There is a perfectly perceptible feeling of dislike to the last two durations." (A4 and A5) But the time of reaction to A4 is really longer than to



A2 and A3. B<sub>2</sub>, B<sub>3</sub>, B<sub>4</sub> and B<sub>5</sub> show a continued lengthening of the reaction time as the duration of the stimulus decreases. The subject reported, however that while there was some difference in intensity between B<sub>4</sub> and B<sub>5</sub> none was noticeable between the other Bs. The graphs show this progressive shortening plainly. They are quite irregular.

TABLE XV

Subject De. 5000 Reactions				
A1(1 sec.)	A2(150 $\sigma$ )	A3(66 $\sigma$ )	A4(31 $\sigma$ )	A5(10 $\sigma$ )
186	172	160	159	175
186	178	162	173	178
184	168	167	164	164
183	166	179	161	164
197	182	180	164	174
184	169	168	155	169
180	162	172	169	178
185	167	170	164	198
178	163	169	173	175
169	171	173	164	178
<hr/>				
Average 183.2	169.8	170.0	164.6	175.3
M. V. 10.46	18.35	11.12	8.46	14.51
<hr/>				
B1(1 sec.)	B2(144 $\sigma$ )	B3(64 $\sigma$ )	B4(25 $\sigma$ )	B5(12 $\sigma$ )
141	192	188	159	180
170	183	185	163	174
183	188	184	180	164
185	187	178	167	163
185	161	176	150	166
170	178	159	161	159
174	182	174	160	167
166	170	172	176	176
180	183	178	167	175
179	177	179	180	183
<hr/>				
Average 173.3	180.1	177.3	166.3	170.7
M. V. 13.01	17.25	14.16	21.28	14.45

TABLE XVI

Subject D. 5000 Reactions				
A1(1 sec.)	A2(150 $\sigma$ )	A3(66 $\sigma$ )	A4(31 $\sigma$ )	A5(10 $\sigma$ )
174	159	153	174	199
174	160	164	163	176
175	169	157	163	174
171	175	165	167	182
178	153	170	167	186
173	162	162	169	171
169	154	174	172	182
173	180	166	165	181
171	170	158	171	184
170	159	178	167	181
<hr/>				
Average 172.8	164.1	164.7	167.8	181.6
M. V. 16.44	19.86	14.35	14.0	16.02

B1(1 sec.)	B2(144 $\sigma$ )	B3(64 $\sigma$ )	B4(25 $\sigma$ )	B5(12 $\sigma$ )
186	168	165	186	186
189	168	177	180	188
189	170	186	188	186
193	176	183	180	180
210	172	187	183	186
198	170	186	180	196
201	171	179	179	188
169	174	178	191	196
175	172	178	190	188
183	166	176	182	183
<hr/>				
Average 189.3	170.7	179.5	183.9	187.7
M. V. 20.66	14.19	14.13	13.29	13.96

Wh. is the same subject who reacted to the auditory stimuli. At the time of the visual work he was a member of the sophomore class of the Johns Hopkins University. His work was interrupted toward the end and he reacted 250 times to A<sub>3</sub> and only 200 times to A<sub>4</sub> and A<sub>5</sub>. Consequently the graphs for these series show very little elevation. See Plates XIX and XX. In Table XVII the averages to A show those to A<sub>4</sub> to be the shortest. The reactions to A<sub>2</sub>, A<sub>3</sub> and A<sub>5</sub> are about the same length. Those to A<sub>1</sub> are much the longest. A very similar result is seen in the B series. Here B<sub>3</sub> has the shortest averages, B<sub>4</sub> somewhat longer, B<sub>2</sub> and B<sub>5</sub> the same length and longer than B<sub>4</sub> and B<sub>1</sub> longer than any.

The graphs are irregular and are very much spread out.

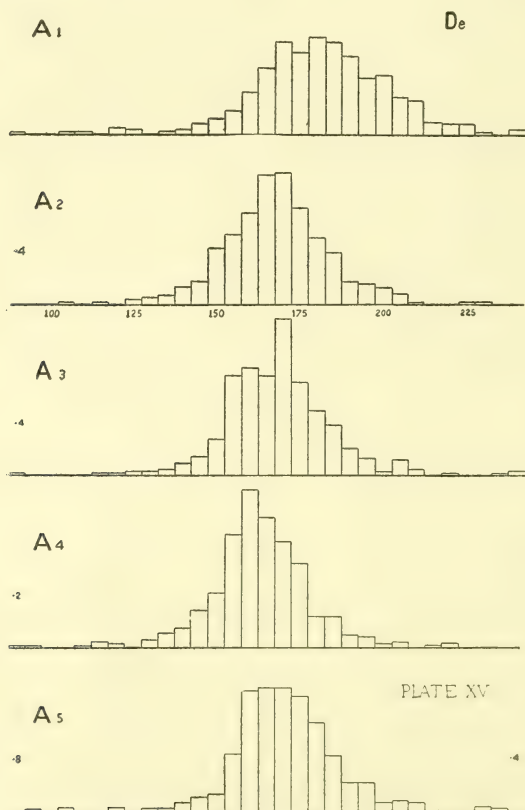
TABLE XVII

Subject Wh. 4150 Reactions

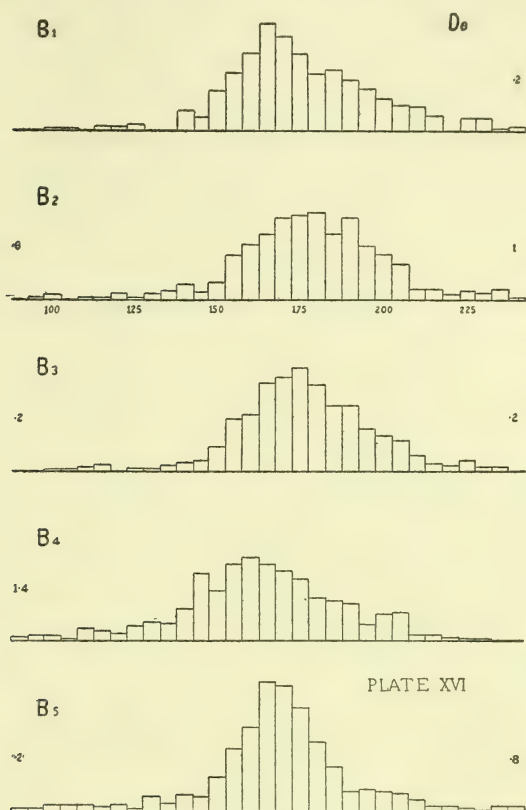
B1(1 sec.)	B2(144 $\sigma$ )	B3(64 $\sigma$ )	B4(25 $\sigma$ )	B5(12 $\sigma$ )
176	200	182	184	198
209	193	185	182	206
206	198	176	199	209
219	201	181	200	193
217	199	196	211	190
218	216	174	189	190
214	212	194	190	207
215	193	183	196	222
216	196	175	193	211
205	203	183	187	203
<hr/>				
Average 209.5	201.1	182.9	193.1	202.9
M. V. 16.7	14.21	20.06	21.69	19.91

A <sub>1</sub> (1 sec.)	A <sub>2</sub> (150 $\sigma$ )	A <sub>3</sub> (66 $\sigma$ )	A <sub>4</sub> (31 $\sigma$ )	A <sub>5</sub> (10 $\sigma$ )
199	199	202	186	190
217	203	203	179	197
221	195	198	190	207
213	196	196	191	199
211	202	205		
209	199			
205	209			
218	207			
197	179			
205	196			
<hr/>				
Average 209.5	198.5	200.8	186.5	198.25
M. V. 15.43	20.03	21.23	26.31	26.25

Wi., a member of the sophomore class of the undergraduate class of the Johns Hopkins University reacted the full five thou-



sand times, according to the program. He reacted first to B; then through the series to B<sub>5</sub>, to A<sub>1</sub>, to A<sub>5</sub>. The reactions are averaged in Table XVIII and graphed in Plates XXI and XXII. His longest average is found in the reactions to A<sub>2</sub> and A<sub>5</sub>. All the reactions to the members of the A series are nearly of the



same length, that to A<sub>4</sub> being the shortest. There is less than ten sigma difference between the most widely separated of the averages. A<sub>3</sub> and A<sub>4</sub>, the stimuli of medium durations, are a little shorter than the others. In Wi.'s reactions to the B series we find the reactions to the extremely long B, and the extremely short B<sub>5</sub> stimuli to be the slowest. The averages of the three medium intensities differ by less than nine sigma.

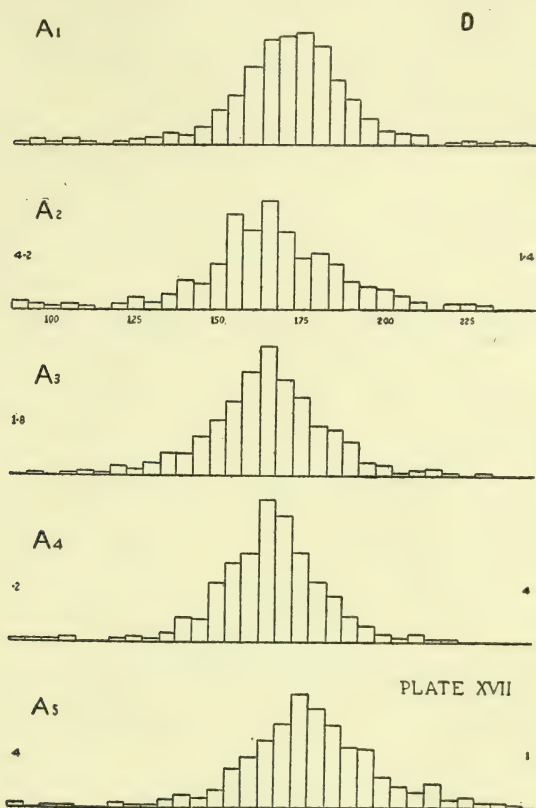
The graphs are somewhat spread out and in no case is there a secondary crest of any importance.

B. is a freshman in the Johns Hopkins University. He reacted to all the stimuli in the same order as did Wi. Here we see the reactions to the longest and the shortest of the A stimuli to be slower than the reactions to the medium durations. The three medium intensities are separated by only eight sigma. For the first time we see secondary crests equal in height to the primary crests in the graphs drawn from the reactions of B. This fact alone may throw some doubt on the value of his reactions. In the B series we find a progressive decrease in the length of reaction time as the duration of the stimulus decreases. This

TABLE XVIII

Subject Wi. 5000 Reactions				
B1(1 sec.)	B2(144 $\sigma$ )	B3(64 $\sigma$ )	B4(25 $\sigma$ )	B5(12 $\sigma$ )
185	191	174	168	187
187	189	170	156	182
186	185	174	167	183
203	174	178	176	195
197	183	180	175	185
193	184	178	180	186
194	180	173	164	197
178	180	178	183	199
176	182	179	181	181
175	173	179	182	186
<hr/>				
Average 187.4	182.1	176.3	173.2	188.1
M. V. 16.42	12.01	14.29	16.33	16.51
<hr/>				
A1(1 sec.)	A2(150 $\sigma$ )	A3(66 $\sigma$ )	A4(31 $\sigma$ )	A5(10 $\sigma$ )
188	194	199	191	189
189	194	196	187	184
197	195	192	190	187
203	200	192	201	203
200	199	190	185	195
196	214	184	189	195
201	189	184	189	207
201	205	188	187	202
186	211	199	192	216
194	202	199	193	220
<hr/>				
Average 195.5	200.3	192.3	190.4	199.8
M. V. 29.26	15.28	17.62	16.75	22.31

would look at first glance like a practice effect. But the subject had a long training series in reacting before any of the recorded reactions were made. And, furthermore, there is no evidence of any practice effect after B5. As soon as the subject had finished reacting to B5 he immediately began reacting to A1. See Table XIX and Plates XXIII and XXIV.



L. is a member of the sophomore class of the Johns Hopkins University. His work was interrupted by sickness before he had half finished. He reacted 500 times to stimuli B1, B2 and B3, 400 times to B4, and 200 times to B5, and all the A series. The averages of his reactions are given in Table XX and the graphs in Plates XXV and XXVI. The difference between the longest and the shortest average in the A series is  $17.55\sigma$ . The

longest averages is to A2. There is one very long daily average included in this total. The graph of A2 is seen to be very much spread out, more so than any of the others, although all the graphs of this subject's reactions are elongated. A1, A3, A4 and A5 are practically of the same length. In the B series B1 and B4 are the longest, B2, B3, and B5 about the same.

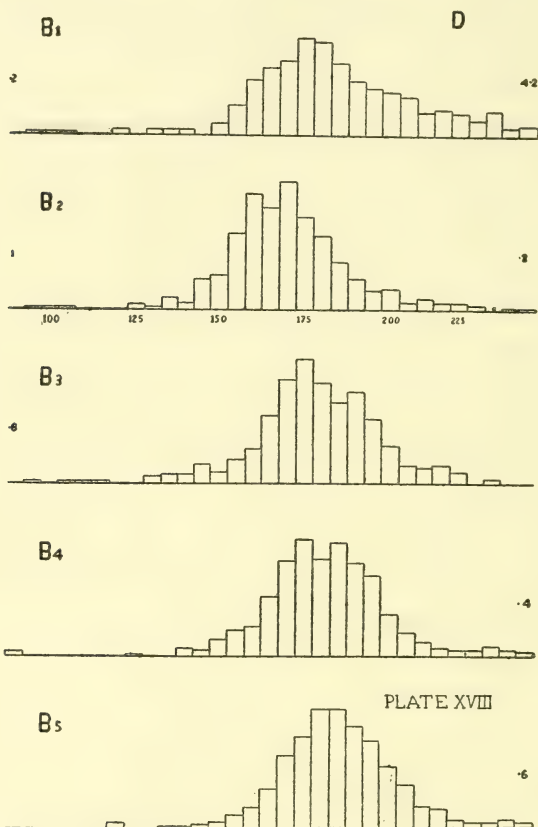


TABLE XIX

Subject B. 5000 Reactions				
B1(1 sec.)	B2(144 $\sigma$ )	B3(64 $\sigma$ )	B4(25 $\sigma$ )	B5(12 $\sigma$ )
195	180	162	171	161
193	179	162	164	163
172	165	165	164	158
200	165	161	161	158
201	176	157	153	154
201	186	170	157	158
191	178	169	157	153
185	176	169	157	150
194	163	169	163	146
177	168	192	156	151
<hr/>				
Average	190.9	173.6	167.0	160.3
M. V.	19.47	16.80	19.11	33.57
<hr/>				
A1(1 sec.)	A2(150 $\sigma$ )	A3(66 $\sigma$ )	A4(31 $\sigma$ )	A5(10 $\sigma$ )
185	139	155	147	169
182	154	154	152	165
175	126	155	160	179
182	174	151	158	176
180	154	170	160	171
178	156	166	166	185
179	155	163	153	183
150	151	152	163	175
132	150	163	154	177
152	149	159	152	175
<hr/>				
Average	169.5	150.8	158.8	156.5
M. V.	21.49	26.07	23.15	14.38

TABLE XX.

Subject L. 3100 Reactions				
B1(1 sec.)	B2(144 $\sigma$ )	B3(64 $\sigma$ )	B4(25 $\sigma$ )	B5(12 $\sigma$ )
205	190	185	214	208
217	199	184	200	205
205	188	190	208	196
216	204	194	203	192
216	201	196	209	
206	196	204	206	
201	199	193	213	
213	200	219	207	
219	204	225		
202	204	205		
<hr/>				
Average	210.0	198.5	199.5	207.5
M. V.	17.05	12.82	21.39	14.87

A1(1 sec.)	A2(150 $\sigma$ )	A3(66 $\sigma$ )	A4(31 $\sigma$ )	A5(10 $\sigma$ )
186	195	204	184	198
192	215	194	193	193
192	209	192	198	190
190	211	186	187	200
<hr/>				
Average 190	207.5	194	190.5	195.25
M. V. 17.98	18.55	15.54	16.11	24.

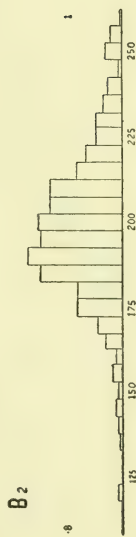
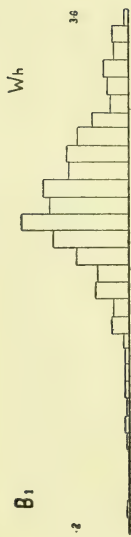


PLATE XIX

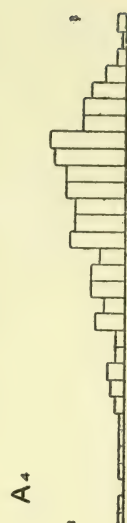
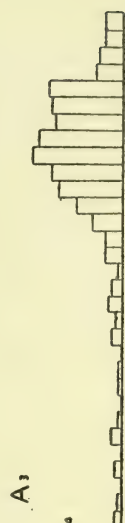
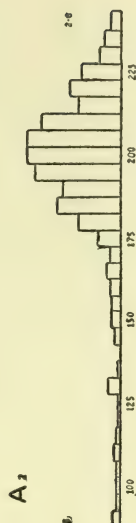
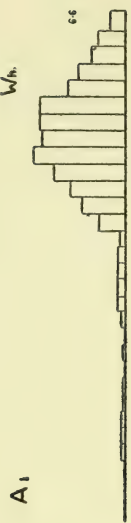


PLATE XX



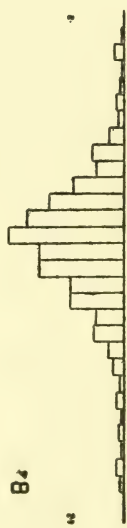
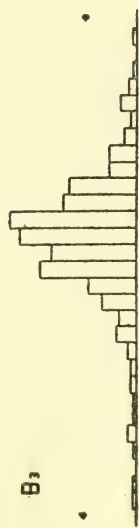
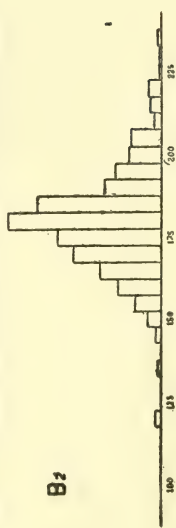
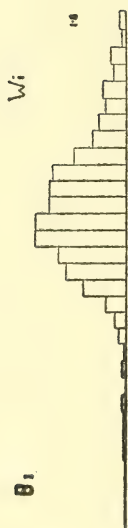


PLATE XXI

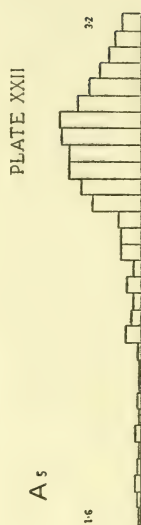
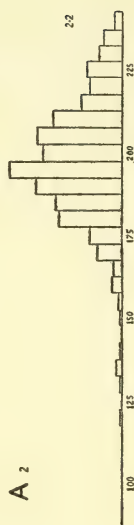


PLATE XXII

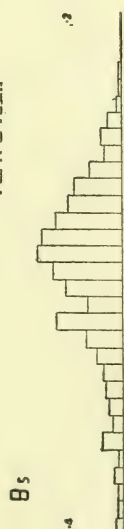
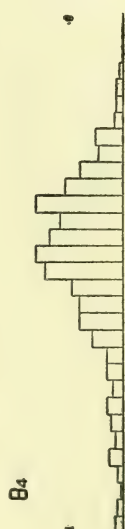
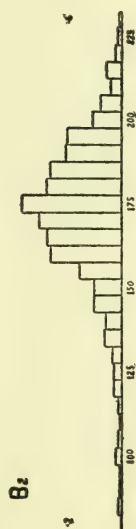


PLATE XXIII

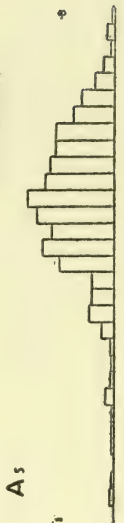
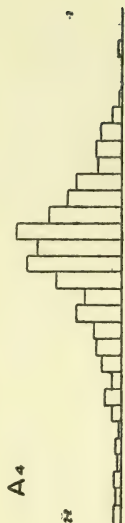
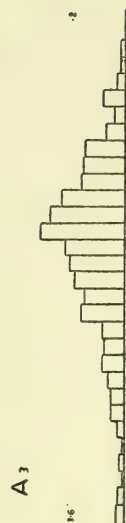
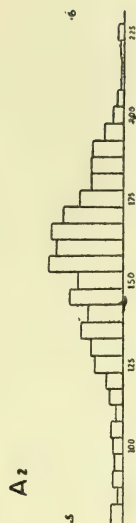


PLATE XXIV

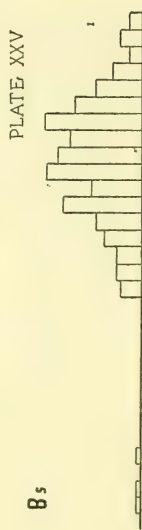
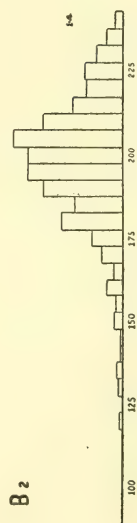


PLATE XXV

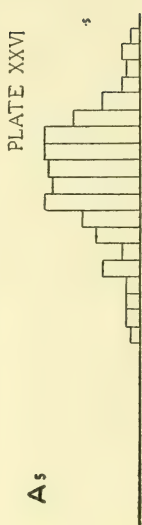
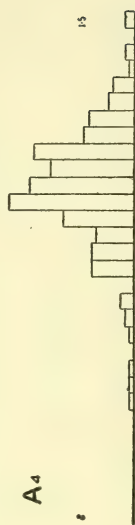
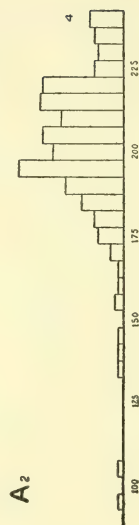


PLATE XXVI

#### IV. CONCLUSION

The results of the foregoing experiments are not easily arranged to establish any definite rule. But they certainly do not support any hitherto promulgated principle of the relation of stimulus to reaction time. They do not follow a rule analogous to Weber's law, as the results which Froeberg obtained apparently did. Nor is there any evidence of increase of reaction time with decrease of stimulus duration. In fact the results, in so far as they are positive, point in an opposite direction. The longer the duration of stimulus the longer does the reaction time tend to be.

The results on the relation of the duration of auditory stimulus to reaction time are, I think, unequivocal. Duration of auditory stimulus of the lengths used does not materially affect reaction time. If an auditory stimulus be sufficiently long to enter consciousness, any prolongation of that stimulus up to the greatest duration we used will not affect the reaction time, provided the intensity be kept constant.

The results of the reactions to visual stimuli are not so clearly uniform. A very small number of *sigma* separates the longest from the shortest average in the results of most of the subjects. This is noteworthy. If the reaction averages of all subjects to each stimulus be themselves averaged the smallness of the differences is more than ever noticeable. Such an averaging is not defensible on methodological grounds, and no deduction can be drawn from it alone, but it furnishes a convenient birdseye view. The results are so averaged in the following table:

TABLE XXI.

	A1	A2	A3	A4	A5	B1	B2	B3	B4	B5
T.....	178.5	172.6	172.2	181.5	189.9	176.6	171.8	169.1	167.8	151.0
S.....	201.5	201.9	205.5	203.3	196.9	198.0	193.9	190.4	181.3	192.9
De.....	183.2	169.6	170.0	164.6	175.3	173.3	180.1	177.3	166.3	170.7
D.....	172.8	164.1	164.7	167.8	181.6	189.3	170.7	179.5	183.9	187.7
Wh.....	209.5	198.5	200.8	186.5	198.2	209.5	201.1	182.9	193.1	202.9
Wi.....	195.5	200.3	192.3	190.4	199.8	187.4	182.1	176.3	173.2	188.1
B.....	169.5	150.8	158.8	156.5	175.5	190.9	173.6	167.0	160.3	155.2
L.....	190.0	207.5	194.0	190.5	195.2	210.0	198.5	199.5	207.5	200.2
	187.6	183.2	182.5	180.2	189.0	191.9	183.5	180.2	179.2	180.9

If in the tables the results of the B series be examined carefully it will be seen that in the case of T. there is a regular decrease in the reaction time as the stimuli become shorter. The total decrease is  $25.6\sigma$ . With S. there is a regular decrease from B1 to B4, B4 being  $16.7\sigma$  shorter than B1. B5 is somewhat longer than the two preceding durations. With De. there is no regular decrease, but B4 is  $7\sigma$  shorter than B1. D. shows very nearly the same decrease as De. In both cases B5 is longer than B4. Wh. makes a regular decrease from B1 to B4 totalling  $16.4\sigma$ . Wi. has very regular decrease from B1 to B4, totalling  $14.2\sigma$ . B. has a decrease of  $35.7\sigma$  from B1 to B5 and L. though he shows no regular decrease, makes the average reaction to B4  $2.5\sigma$  shorter than that to B1. Thus in the B series five subjects show a regular decrease in reaction time as the duration of the stimulus decreases. The other three subjects react more quickly to B4 than they do to B1. The successive average decrements are small, but inasmuch as they are based upon a very large number of reactions, and as they are found in five subjects, and are, at least, not contradicted by the other three subjects, they cannot be without significance.

The results from the A series while they do not strongly substantiate the suggestion of the B series, certainly do not contradict it. In five cases A4 is shorter than A1. In one case, that of T., A4 is  $3\sigma$  longer than A1. In the case of S. A4 is  $1.8\sigma$  longer than A1 and in the case of L. A4 is  $.5\sigma$  longer than A1.

It is not possible that the progressive shortening is due to the effect of practice. Each subject underwent a thorough training in reacting before any reactions were recorded. This preliminary series was kept up until the shortening effect had disappeared. And then it will be seen by reference to the tables that there is no improvement to be seen within the series of reactions to any one stimulus.

A5 and B5 do not fit into the suggested rule of progressive decrease of reaction time to decrease in length of stimuli. The reason for this probably lies in the fact that the intensity of A5 and B5 was less than the intensity of the other stimuli. That a decrease in intensity of stimulus increases the time is established.

Wundt<sup>60</sup> goes into the question in some detail. He says that the length of the reaction time varies in general inversely as the intensity of the stimulus. He found that the reaction time was longest in the neighborhood of the stimulus threshold. With increasing strength of visual stimulus the reaction time at first decreased very fast, but more slowly in reacting to medium intensities. There were apparently wide limits in which there was little or no change in reaction time. With auditory stimuli of different strengths the same general law was found to exist. Von Kries and Auerbach<sup>56</sup> obtained a wider application of the same law with the use of electrical stimuli. They found a rapid decrease of reaction time corresponding to increase of strength of weak intensities. Wundt holds that as the intensity still further increases the reaction time will begin to increase. Exner<sup>28</sup> found different results on this point. He believed that a very strong stimulus which caused a shock produced a great shortening of the reaction time. Von Kries and Auerbach found that discrimination time was shorter for strong stimuli than for weak. Berger<sup>12</sup> investigated the validity of Wundt's law. He obtained six intensities of visual stimulus by inserting grey glasses between the eye and the source of light, and two higher intensities were produced by using a condensing lens. He found a very regular decrease of reaction time as the intensity of the stimulus increased. These results were almost paralleled by reactions to electrical stimuli. Reactions to four different intensities of auditory stimuli produced by letting a ball drop from the Hipp Fall-apparatus at heights of 60, 160, 300 and 500 mm. gave less regular results.

Berger concluded that reaction time varies inversely as the intensity of the stimulus, more markedly the nearer we approach to the stimulus threshold. He holds this to be true not only for sound stimuli as pointed out by Wundt and for electrical stimuli as investigated by Von Kries and Auerbach, but also for light stimuli. Berger asserts that it seems probable that Wundt's statement that reaction time increases with stimuli of very strong intensity is true, although he admits that his own investigation does not prove it.

The number of subjects used in this investigation was entirely inadequate, there being but one, J. M. Cattell, beside Berger himself.

This fact, and the fact that each of these two observers reacted only 150 times to each intensity detracts from the significance of the results of this investigation.

Cattell and Dolley<sup>15</sup> working on reaction time to dermal stimuli of various kinds (electrical, touch, etc.) found that "the time of reaction was shorter when the intensity was greater".

G. Martius,<sup>42</sup> investigated the influence of the intensity by using the tones C' C c' c''' c'''' and a noise as stimuli using two intensities of each, a strong and a weak. He comes to the conclusion that it is not strictly accurate to say that reaction time decreases with increasing intensity of impression, for practice and attention will equalize reaction times to different intensities of tones within the scale used.

Martius acted as subject in this experiment with one other subject. The experiments never numbered more than 20 for any particular stimulus and frequently were as few as 11. They are entirely too inadequate in number to justify any positive assertions based upon them.

Slattery<sup>50</sup> measured the time of reaction to (1) different intensities of tone, (2) tones of different pitch (3) electrical stimuli of different intensities. He sums up his results on intensity of tone by saying that reaction time does not vary with intensity of stimulus in any degree that can be detected, thereby disagreeing with Wundt and the majority of observers, but agreeing with Martius.

It should be said that the number of subjects and of actual reactions recorded by Slattery in the investigation was too small to be a sufficient basis for any generalization whatsoever, and especially so for a generalization which is somewhat revolutionary in its nature.\*

\* It seems to the present writer to be a waste of space to print and of time to read any article based on such a paucity of experimental material as is reported in this article and in that of Martius. Nor are these two writers alone in this fault. Far too many men have rushed into print to enunciate with all confidence conclusions based upon an utterly inadequate number of observations.

Froeberg,<sup>32</sup> in the article referred to above, worked with variations of the intensity and of the magnitude of stimulus as well as of duration. He worked with visual and auditory stimuli. Effects of variation of intensity on time of reaction was first investigated. This investigation was divided into two parts. In the first part the intensity and the size of the stimulus were varied and in the second the intensity and duration. The greatest intensity was the Milton-Bradley white baryta paper designated as 100. The other intensities were obtained from Hering's greys, and their relative brightness tested by the size of a white sector on a color mixer which produced exactly the same shade. Four were chosen besides that of 100, namely 56, 25, 16 and 10. Four subjects were used, each reacting 400 times to each intensity. From the results the author concludes that the time of reaction tends to increase arithmetically as the intensity of the stimulus decreases geometrically.

The effect of the variation of the size of the stimulus on the time of reaction was next investigated. Five sizes of stimulus were used.

The results of these tests show that the time of reaction increases with decreasing size of the stimulus. Over a limited range this increase is made by approximately equal arithmetical increments as the size of the stimulus decreases geometrically, but as the threshold is approached the increase becomes more rapid.

The experiments with auditory stimuli of different intensities obtained by dropping a steel ball from different elevations lead Froeberg to the conclusion that the time of reaction increases with decreasing intensity of sound. On account of the lack of a reliable measure of the physical intensity of sound no definite relation of the intensity to the reaction time can be stated, but it can be said that there is a general inverse relation.

The text-books in general take the ground that as a general rule reaction time varies inversely as the intensity of the stimulus.\*

\* James, "Principles of Psychology" vol. 3, p. 96; Külpe, "Outlines of Psychology" p. 407; Ladd and Woodworth, "Elements of Physiological Psychology" p. 479.

While there is some disagreement as to the exact meaning of the experiments with different intensities of stimuli (as between Külpe on one hand and Cattell and Dolley on the other as regards the effect of different intensities of electrical-cutaneous stimuli) yet the final conclusion that in the realm of moderate intensities the time of reaction varies inversely as the intensity of the stimulus, is the general conclusion of most observers

According to the foregoing investigations, which seem to have established the rule that decrease in intensity produces increase in reaction time, we may reasonably assume that the extreme shortening of stimulus-duration may, through the consequent reduction of intensity, have resulted in producing a prolonged reaction to A<sub>5</sub> and B<sub>5</sub>.

It is very probable that reactions to visual stimuli of various durations vary, not inversely, but directly with the durations of those stimuli, but not in the same ratio. We are more accustomed to react quickly to short auditory impressions than we are to short visual impressions. Perhaps this is why the results to the auditory stimuli are negative.

Froeberg's results with visual stimuli do not agree with mine. His subjects presumably were all acquainted with the effect of intensity variations on reaction time. They may have expected the results which they finally obtained. At least this possibility is not excluded in Froeberg's paper.

The subjects of my experiment with visual stimuli were, with one exception, absolutely naïve. They knew nothing at all of the effect of intensity upon reaction time. Their reactions were not modified by pre-conceptions of any sort. Dr. Dunlap, of course, was an exception. But he was careful not to learn the result of his own reactions nor those of the other subjects until he had finished reacting.

In the auditory series, which was completed before the visual work was commenced, three subjects had some knowledge of the results of work with varying intensities of stimulus. Obviously, this did not affect their results.

The original problem which suggested the present experiment

was that of the investigation of the characteristic difference of reaction time to stimuli of the visual and auditory modes.

The explanation of this difference which is most generally adopted is that there is a certain latency in the response of the visual end organs, due to the fact that the photochemical process is quite slow. It was partly to investigate this that the series was introduced having the disappearance of a light as stimulus. The shortest disappearance or occlusion stimulus used was  $10\sigma$ . In this case there was a continuous visual stimulation, suddenly and completely interrupted, and reinstated  $10\sigma$  later. The interruption of the light was plainly perceivable, no subject had any difficulty whatsoever in seeing it. As the terminal lag of the visual sensory process is demonstrably longer than the initial lag, the total time of the drop in the process, when the stimulus is cut out and then readmitted  $10\sigma$  later, must be less than  $10\sigma$ . Probably the rise in the process commences at the point at which the stimulus is readmitted: but at any rate the process cannot continue to fall beyond that point. A distinctly noticeable change in the process occurs, therefore, within  $10\sigma$  or less after the occlusion of the stimulus. The delay of the appearance of a perceptible change in visual sensation due to the lag of the sensory process cannot, therefore, in this case account for even a  $10\sigma$  lengthening of the visual reaction as compared with the auditory reaction—there being presumably some lag even in the auditory process. Since, as said above, the visual process rises more quickly than it falls, and since, moreover, the reactions to the occlusion of light differ little from the reactions to the appearance of light, we must conclude that in this case also, the mere delay in the appearance of the change cannot account for the lengthening of the reaction.

Inasmuch as the energy of even a very moderate auditory stimulus is much greater than that of a strong visual stimulus the characteristic difference may be explained by this fact.

The results of the present investigation suggest another possible explanation. We are far more accustomed to react to short sharp auditory stimuli than to equally short visual stimuli. Practically all visual stimuli exerts a more prolonged effect upon the

rods and cones than an auditory stimulus does upon the organ of Corti. This investigation has made it probable that reactions are slower to visual stimuli of a somewhat prolonged nature than to shorter stimuli. It should be noted that in this investigation the subjects completed reacting to all the stimuli of a certain duration before they reacted to the stimuli of another duration. With our apparatus it is impossible to mingle the stimuli of various durations indiscriminately. Therefore, the subjects had a certain adaptation to the length of stimulus which was to be reacted to. This adaptation is what affected the length of reactions. Exactly the same kind of adaptation is present when reacting to any stimulus. If the stimulus is visual a certain length is expected. It may be that it is this fact which prolongs reactions to visual stimuli over and above reactions to auditory stimuli.

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## The Influence of Stimulus Duration on Reaction Time

By

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in Oberlin College

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